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SCIENCE BYWAYS.

Faithfully yours
Richd A. Procto̅
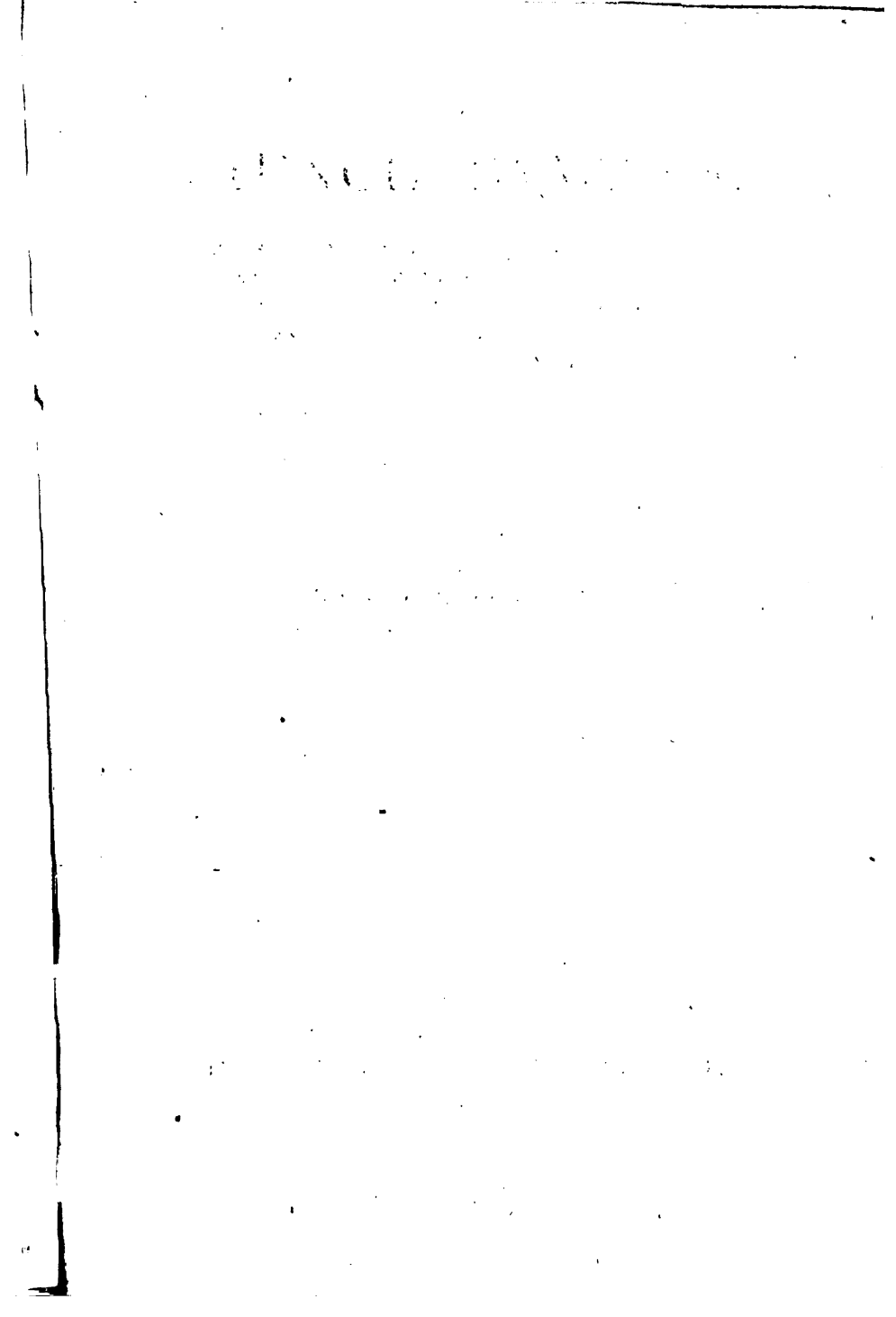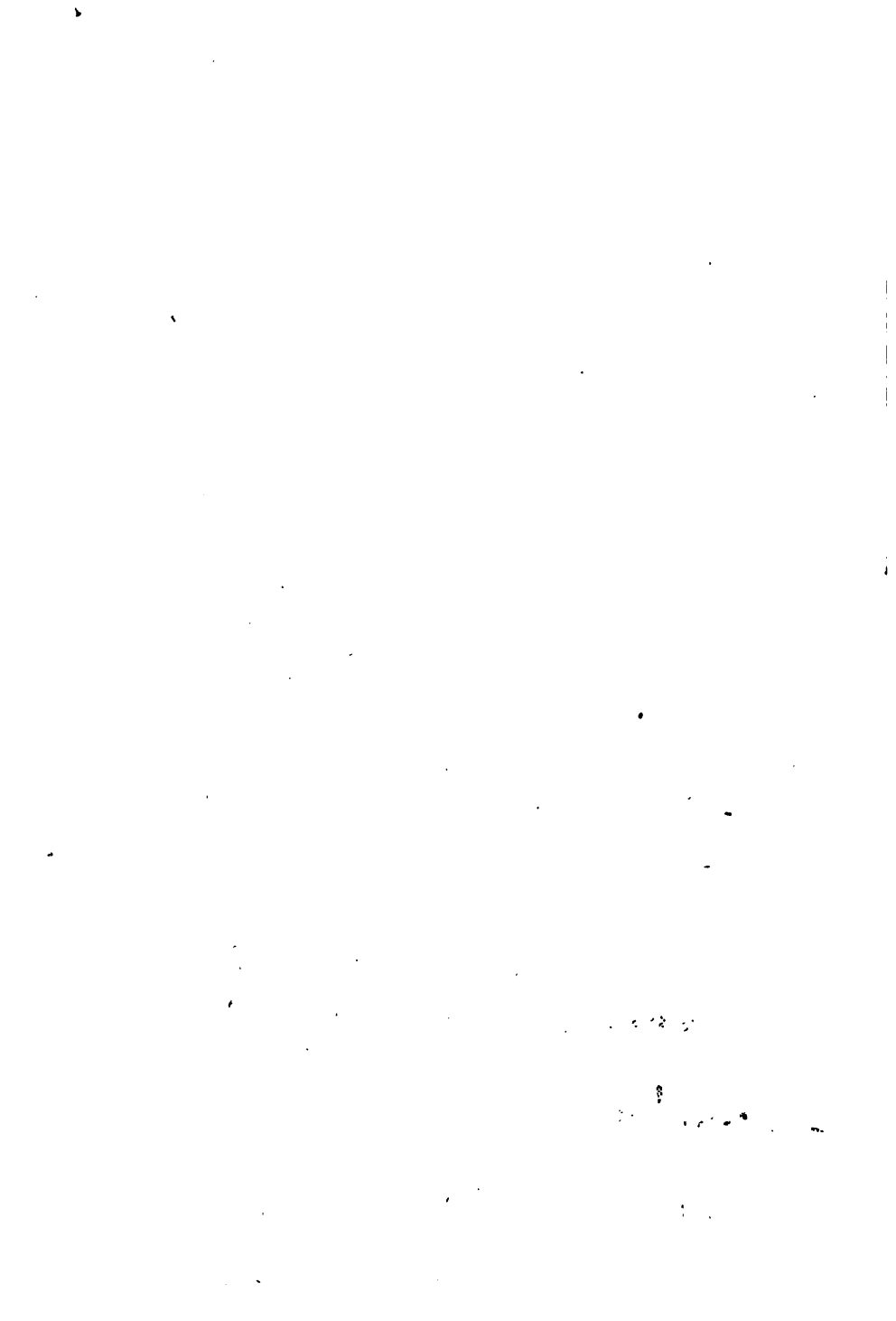
SCIENCE BYWAYS:

A SERIES OF FAMILIAR DISSERTATIONS ON LIFE IN OTHER
WORLDS; COMETS AND THE SUN; THE NORTH POLE;
RAIN; DANGER FROM LIGHTNING; GROWTH
AND DECAY OF MIND; THE BRAIN AND
MENTAL FEATS; AUTOMATA; &c.

TO WHICH IS APPENDED AN ESSAY ENTITLED

MONEY FOR SCIENCE

BY

RICHARD A. PROCTOR,

AUTHOR OF 'SATURN,' 'THE SUN,' 'THE MOON,' 'THE UNIVERSE,' ETC.

Curiously I roam
As through a wide museum,—from whose stores
A casual rarity is singled out
And has its brief perusal, then gives way
To others, all supplanted in their turn.

WORDSWORTH.

WITH A PHOTOGRAPHIC PORTRAIT OF THE AUTHOR.

LONDON:

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1875.
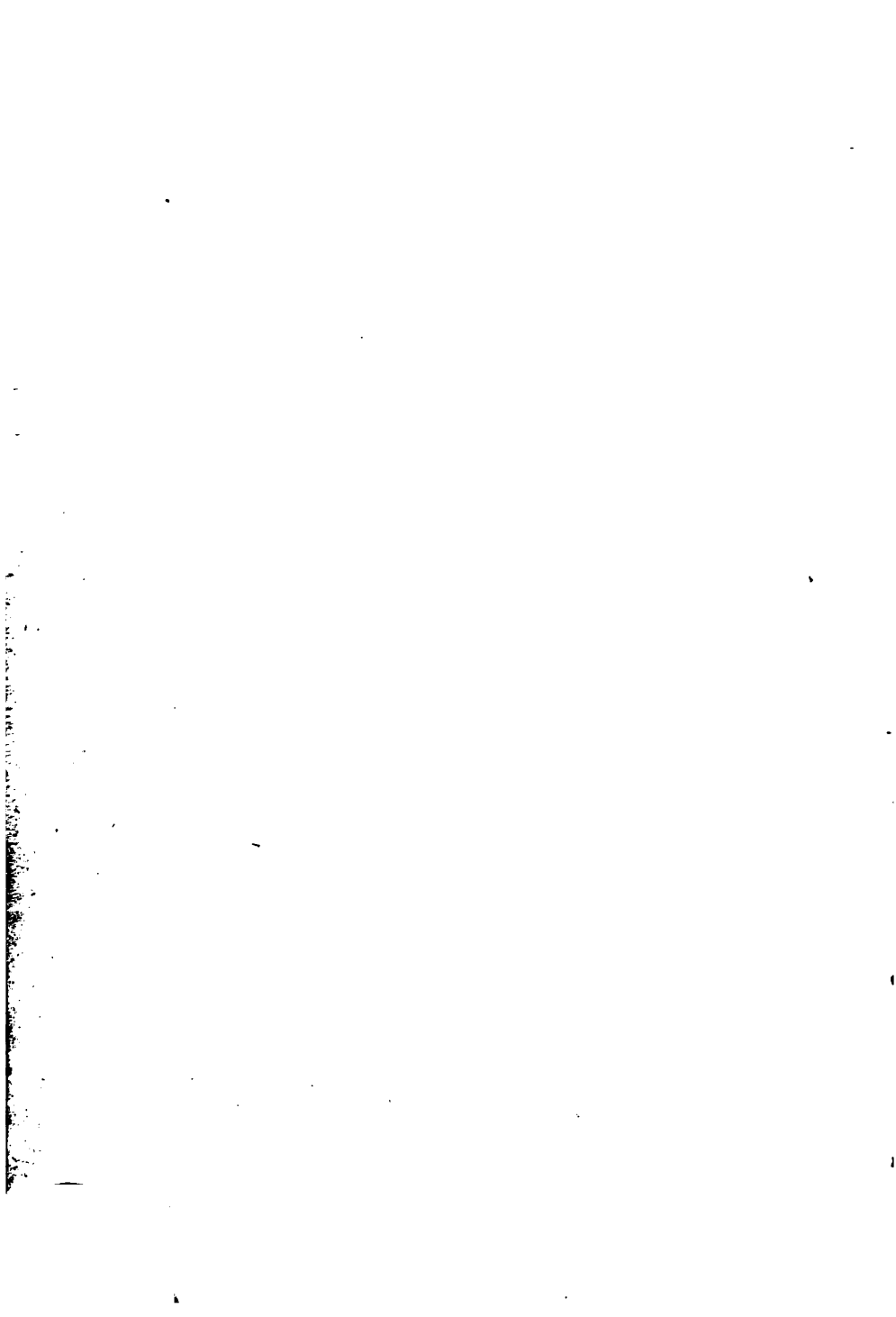
PREFACE.

As the title of my 'Borderland of Science' was misunder-
stood by some, it may be well perhaps for me to explain
in what sense I apply the term 'Science Byways' to the
essays of the present volume. In the study of science
there are three chief departments :—(1) original research,
which may be compared to the pioneering of new roads ;
(2) the thorough study of a branch of science, in all its
details, with such observations, experiments, and calcula-
tions as may be necessary to give a complete mastery of
it,—such study being comparable to travel on the high
road, and providing the only trustworthy approach to
the lines on which new roads are to be pioneered; and
thirdly, the study of scientific facts without the mastery
of the methods or processes by which they have been
ascertained,—a form of study which may be compared to
wandering on pleasant by-paths beside the regular road-
way. And, as there can be no reason why the voyager

who leaves the beaten track, having perhaps no special
reason for pursuing it, should not obtain true views of
the country through which the highway runs, so there
can be no reason why the student of science who has
no time or inclination for the mastery of special depart-
ments of research, should not obtain exact ideas about
scientific discoveries. False views and fanciful illustra-
tions are no more needed to render the study of science
attractive, than rose-tinted glasses to make the varying
landscape please the eye of one who wanders through
meadow or lane, by the bed of rivulet or under the
green shade of trees, in pleasant valleys or over upland
slopes, instead of plodding (as the business-traveller must)
along the highway's dusty level. It is necessary, indeed,
to have a guide along the byways of science, who not
only knows the true highway, but has surveyed the
country around. For otherwise the views obtained from
various points along the pleasant ways will give false
ideas of the country through which the pathways run,
even if the voyager does not wholly lose his way. But
granting such a guide, as true views of scientific dis-
covery may be obtained in science byways as along the
beaten path; though as I have said, everyone who desires
to pursue original researches must himself follow the
regular highway.

Many, also, who are themselves scientific investigators

must be content to follow science byways in other sub-
jects than those specially their own.   For such, quite as
much as for the general reader, the so-called popular
treatises on science are written, and by such are popular
treatises read.   Nor can anyone write really useful
popular treatises on science who has not himself un-
dertaken scientific researches.   I do not say that no man
can write usefully on a scientific subject which he has
not himself thoroughly mastered.   The student of science
can readily grasp the significance of facts in other depart-
ments than his own : and thus a Humboldt may write
effective popular essays on astronomical subjects, a Her-
schel on meteorological subjects, a Tyndall on geological
subjects, an Agassiz on physical subjects, a Carpenter on
oceanic circulation, though their several branches of re-
search were other than those thus occasionally dealt with.
In such cases the scientific habit of mind ensures accuracy
in the mode of treatment.

A vague objection exists in some minds against popu-
lar treatises, as though the popular treatment of a subject
necessarily involved inaccuracy.   This is doubtless due
in great part to the fact that many who know little of
science have undertaken to write such treatises.   But in
reality a popular treatise may be as rigidly exact in all
statements of fact as a work too technical to find any
readers save among special students of its subject.   It

is as accurate to say that 'the earth, seen from the sun, would appear no larger than a one-inch ball seen from a distance of 320 yards,' as to say that 'the horizontal solar parallax amounts to 8·9 seconds of arc'; but whereas every intelligent person sees the significance of the first statement, the second, which has the same real meaning, will be understood only by the student of astronomy.

Some also imagine that there may be a tendency to sensationalism in popular works on science. But the writer who is familiar with the true nature of modern scientific researches needs no help of that kind. He must indeed be a man of dull and heavy mind under whose pen the marvels of modern science become unattractive. The real lover of science should indeed be ashamed to attempt fine writing, even as one who desired to exhibit the real beauties of a noble work of art should be ashamed to deck it with gaudy ornaments.

Nevertheless, a certain freedom may be permitted, in treatises like the present, for the introduction of matters which in a text-book or exposition of scientific principles would be out of place. Thoughts suggested by scientific discoveries may be touched on, even though fanciful, so only that they are not presented as scientific realities. Speculations, more or less based on evidence, may be suggested for consideration. Theories may be discussed, and the probabilities for and against them may be weighed,

even though as yet the evidence may be insufficient to decide between them. And, *à fortiori*, where the evidence is really strong in favour of particular theories, these may be advocated with advantage. So long as the true position of such fancies, speculations, and theories, is indicated, the interest they excite is useful. Indeed, the enunciation of such ideas and theories can be as readily employed to indicate the limits of the known, as to suggest thoughts of the unknown.

A word now as to some points which have been touched on by reviewers of such works of mine as the present. On the whole, I have had every reason to be more than satisfied with the treatment I have received at the hands of critics, not only of such works, but of my set treatises. But one or two journals in which liveliness appears to be preferred to fairness, have been somewhat perverse in their comments. For instance, when my 'Light Science' appeared, a writer in one of these journals commented on the conceit which could alone lead me to publish in a collected form all the essays I had written. To show the injustice of this charge, I mentioned in the preface to my next work of the kind, the fact, that in reality I have published but a small proportion of my essays. On this, the same journal charged me with boasting of the quantity of matter I had written. As the reviewer affirmed the accuracy of

my writings on popular science, I cannot suppose that
there was any personal feeling in either charge. But it
is not right, for the mere sake of saying something
sprightly and sarcastic, to prefer unjust charges; and of
two inconsistent charges one *must* be unjust. Having
mentioned their nature, I may simply leave these charges
to negative each other.

A more serious (or at least a more seriously ex-
pressed) charge, is brought against me by a reviewer
in the 'Atlantic Monthly,'—the study of whose agreeable
criticisms probably suggested to Wendell Holmes (one of
the ablest contributors to that magazine,) the remarks
quoted at page 279 of the present work. With that
knowledge of personal history which surprises us in
some American publications, this reviewer has discovered
that having gained a large sum of money by the sale of
my treatise on Saturn, I determined forthwith to trade
on what he calls my 'scientific reputation,' by writing
'trash' like my 'Other Worlds Than Ours,' 'Other Suns
Than Ours,' and so forth. He even pictures me as pre-
senting these volumes to the public much as a highway-
man presents a pistol at some unfortunate traveller. I
should have supposed this scarcely an experiment to be
successfully tried; certainly not one which could be re-
peated 'more than twenty' times (as my critic asserts)
in the course of five or six years. I know no way of

forcing· the British public to take books, and I have
seen nothing in America to suggest that authors there
use revolvers and double-barrelled shot-guns to aid the
circulation of their works. In fact, but that my re-
viewer clearly knows much more about my affairs than
I do myself, I should have said that in the last five or
six years I had not produced 'more than twenty,' but
only six or seven books of the kind described; that my
'Saturn' had not brought me in, but had cost me, a large
sum of money;[1] that the work 'Other Suns than Ours,'
which he so definitely reviles, is as yet in my desk in
MS. and not half finished; while the other book named
above, instead of being·written because 'Saturn' had suc-
ceeded, was my first successful work, and was long kept
back because 'Saturn' had failed. But in any case I can-
not understand my reviewer's objection to an author's
gaining money, if he can, by writing about science or
on any other subject. Surely my reviewer was paid for
his two pages of criticism upon my books. If he does not
object to receiving money for reading and reviewing
scientific works, or, (as when they chance to be unwritten)

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[1] As a mere matter of fact I may mention that an edition of 1,000
copies of 'Saturn' was printed early in 1865, of which about 100 copies
have been given away. Considering that the edition is not yet sold off,
it will be easily understood that I have not been very sorely tempted
by the success of that particular work. I shall certainly never try the
experiment of a second edition.

for reviling them without reading them, why should he be angry with me when the works I offer to the public chance to do more than clear their expenses? Such virtue, I must confess, is above my comprehension. Even as respects the progress of science and the spread of scientific knowledge, I should have thought a popular book read by thousands must be more useful than a more profound book read only by a few students. And how have I offended, that I should be condemned to write books of the latter kind only, at a loss of hundreds on each? It is in fact only by writing popular books, that anyone who cannot afford heavy losses is able to provide for the expense of less attractive books, intended for persons entering thoroughly on the study of his subject. In each way, I think, science gains — so that both lines of work are carefully and zealously pursued.

In the essay entitled ' Money for Science,' I deal with another and (I conceive) less satisfactory way of making science—regarded as a vocation—remunerative to science-workers. In a few weeks a little book will appear in which the wants of scientific men in this respect are more fully dealt with, and experiences of my own touched on which may perhaps be of use to some who desire to enter upon scientific pursuits.

RICHARD A. PROCTOR.

LONDON: *October* 1875.

CONTENTS.

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SCIENCE BYWAYS.

LIFE, PAST AND FUTURE, IN OTHER WORLDS.

DURING the summer months of 1875 two planets were
conspicuous which illustrate strikingly the varieties of
condition distinguishing the members of the solar system
from each other. One was the planet Jupiter, at his
nearest and brightest in the middle of April, but con-
spicuous as an evening star for several months there-
after; the other was the planet Mars, shining with
chief splendour towards the end of June, but dis-
tinguishable by his brightness and colour for several
weeks before and after that time. I had occasion to
consider these two planets in three essays in the
Borderland of Science. The first called 'Life in Mars,'
written early in May 1871, dealt with the theory that
life probably exists in Mars. This theory, which may
be called the Brewsterian theory, was not viewed un-
favourably in the essay; for in fact I at that time re-
garded the theory as on the whole more probable than
Whewell's. The second essay, which related to the
planet Jupiter, and bore the title 'A Giant Planet,' was

written early in May 1872. In this essay, the largest
of all the planets was certainly not presented as the
probable abode of life, though, on the other hand, the
theory advanced respecting Jupiter could hardly be
called a Whewellite theory. For Whewell, as the reader
doubtless remembers, advanced the theory that the
globe of Jupiter probably consists in the main of water,
with perhaps a cindery nucleus, and maintained that if
any kind of life exists at all in this planet, its in-
habitants must be pulpy, gelatinous creatures, living in
a dismal world of water and ice ; whereas I pointed to
evidence showing that an intense heat pervades the
whole globe of Jupiter, and causes disturbances so tre-
mendous that life would be impossible there even if we
could conceive the existence of creatures capable of en-
during the planet's fiery heat. Yet a year later I wrote
a Whewellite essay on Mars, in which I dealt with
certain considerations opposed to the Brewsterian theory
that life probably exists on the ruddy planet. Without
absolutely adopting Whewell's view, I discussed those
facts which ' would certainly not be left untouched by
Whewell if he now lived and sought to maintain his
position against the believers in more worlds than
one.'

Those three essays illustrate, but do not strictly
synchronize with, the gradual change in my ideas re-
specting the subject of life in other worlds. In fact, so
far back as the close of the year 1869, I had begun to re-
gard doubtfully the theory of Brewster, which until then
had appeared to me on the whole the most reasonable

way of viewing the celestial bodies. The careful study
of the planets Jupiter and Saturn had shown that any
theory regarding them as the abode of life (that is, of any
kind of life in the least resembling the forms we are
familiar with) is altogether untenable. The great dif-
ference between those planets and the members of the
smaller planetary family of which our earth is the chief,
suggested that in truth the major planets belong to
another order of orbs altogether, and that we have as
much or as little reason for comparing them to the
sun as for comparing them to the earth on which we
live. Nevertheless, in the case of Venus and Mars, the
features of resemblance to our earth predominate
over those of dissimilarity; and it was natural that,
while rejecting the theory of life in Jupiter or Saturn
as opposed to all the available evidence, I should still
consider the theory of life in Mars or Venus as at
least plausible. Ideas on such subjects are not less
tenacious than theories on matters more strictly scien-
tific. Not only so, but the bearing of newly recognised
facts on long-entertained theories is not at once recog-
nised even by those most careful to square their opi-
nions according to the evidence they are acquainted
with. Again and again it has happened that students
of science (in which term I include the leaders of
scientific opinions) have been found recording and ex-
plaining in one chapter some newly recognised fact,
while in another chapter they have described with ap-
proval some old theory, in total forgetfulness of the fact
that with the new discovery the old theory has become

altogether untenable. Sometimes the incongruity is
not recognised until it has been pointed out by others.
Sometimes so thoroughly do our prepossessions become
' bone of our bone and flesh of our flesh ' that even the
clearest reasoning does not prevent the student of
science from combining the acceptance of a newly-dis-
covered fact with continued belief in a theory which
that fact entirely disproves. Let the matter be ex-
plained as it may, it was only gradually that both the
Brewsterian and Whewellite theories of life in other
worlds gave place in my mind to a theory in one sense
intermediate to them, in another sense opposed to both,
which seems to accord better than either with what we
know about our own earth, about the other members of
the solar system, and about other suns which people
space. What I now propose to do is to present this
theory as specially illustrated by the two planets which
adorned our evening skies during the summer months of
the present year.

But it may be asked at the outset, whether the
question of life in other worlds is worthy of the attention
thus directed to it. Seeing that we have not and can
never have positive knowledge on the subject, is it to be
regarded as, in the scientific sense, worthy of discussion
at all ? Can the astronomer or the geologist, the phy-
sicist or the biologist, know more on this subject than
those who have no special knowledge of astronomy, or
geology, or physics, or biology ? The astronomer can
say how large such and such a planet is, its average
density, the length of its day and its year, the light-re-

flecting qualities of its surface, even (with the physicist's aid) the nature of the atmosphere surrounding it, and so on ; the geologist can tell much about the past history of our own earth, whence we may infer the variations of condition which other earths in the universe probably undergo ; the physicist, besides aiding the astronomer in his inquiries into the condition of other orbs, can determine somewhat respecting the physical requirements of living creatures ; and the biologist can show how the races inhabiting our earth have gradually become modified in accordance with the varying conditions surrounding them, how certain ill-adapted races have died out while well-adapted races have thriven and multiplied, and how matters have so proceeded that during the whole time since life began upon our earth there has been no danger of the disappearance of any of the leading orders of living creatures. But no astronomer, or geologist, or physicist, or biologist, can tell us anything certain about life in other worlds. If a man possessed the fullest knowledge of all the leading branches of scientific research, he would remain perfectly ignorant of the actual state of affairs in the planets even of our own system. His ideas about other worlds must still be speculative ; and the most ignorant can speculate on such matters as freely as the most learned. Indeed the ignorant can speculate a great deal more freely. And it is *here*, precisely, that knowledge has the advantage. The student of science feels that in such matters he must be guided by the analogies which have been already brought to his knowledge. If he re-

jects the Brewsterian or the Whewellite theory, it is
not because either theory is a mere speculation for which
he feels free to substitute a speculation of his own ; but
because, on a careful consideration of the facts, he finds
that the analogies on which both theories were based
were either insufficient, or were not correctly dealt with,
and that other analogies, or these when rightly viewed,
point to a different conclusion as more probable.

Nor need we be concerned by the consideration that
there can be no scientific value in any conclusion to
which we may be led on the subject of life in other
worlds, even though our method of reasoning be so far
scientific that the argument from analogy is correctly
dealt with.   If we look closely into the matter, we shall
find that as respects the great purposes for which science
is studied, it is as instructive to think over the ques-
tion of life in other worlds as to reason about matters
which are commonly regarded as purely scientific.   It is
scientific to infer from observations of a planet that it has
such and such a diameter, or such and such a mass ; and
thence to infer that its surface contains so many millions
of square miles, its volume so many millions of cubic
miles, its mass so many billions or trillions of tons; yet
these facts are not impressive in themselves.   It is only
when we consider them in connexion with what we
know about our own earth that they acquire meaning,
or at least that they have any real interest for us. For
then alone do we recognise their bearing on the great
problem which underlies all science,—the question of

the meaning of the wonderful machinery at work around us; machinery of which we are ourselves a portion.[1]

In suggesting views respecting Jupiter and Mars unlike those which have been commonly received with favour, it is not by any means my purpose, as the reader might anticipate, to depart from the usual course, of judging the unknown by the known. Although that course is fraught with difficulties, and has often led the student of science astray, it is in such inquiries as the present the proper, one may almost say the only, course. The exception we take to the ordinary views is not based on the fact that too much reliance has been placed on the argument from analogy, but that the argument has been incorrectly employed. A just use of the argument leads to conclusions very different from those commonly accepted, but not less different from that theory of the universe to which Whewell seems to have felt himself driven by his recognition of the illogical nature of the ordinary theory respecting the plurality of worlds.

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[1] It has often seemed to us that a description, by the close observer Dickens, of the fancies of a brain distempered by fever, corresponds with feelings which the student of science is apt to experience as the sense of the awful mystery of the universe impresses itself on his soul :— '' The time seemed interminable. I confounded impossible existences with my own identity. . . . I was as a steel beam of a vast engine, clashing and whirling over a gulf, and yet I implored in my own person to have the engine stopped, and my part in it hammered off.' Of all the wonders that the student of science deals with, of all the mysteries that perplex him, is there aught more wonderful, more perplexing, than the thought that he, a part of the mighty machinery of the universe, should anxiously inquire into its nature and motions, should seek to interpret the design of its Maker, and should be concerned as to his own share in the working of the mysterious mechanism ?

Let us consider what the argument from analogy really teaches us in this case.

The just use of the argument from analogy requires that we should form our opinion respecting the other planets, chiefly by considering the lessons taught us by our own earth, the only planet we are acquainted with. Indeed, it has been thus that the belief in many inhabited·worlds has been supported; so that if we employ the evidence given by our own earth, we cannot be said to adopt a novel method of reasoning, though we may be led to novel conclusions.

The fact that the earth is inhabited, affords, of course, an argument in favour of the theory that the other planets are also inhabited. In other words a certain degree of probability is given to this theory. But we must look somewhat more closely into the matter to ascertain what that probability may amount to. For there are all orders of probability, from certainty down to a degree of probability so low that it approaches closely to that extremest form of improbability which we call impossibility. It is well at once to take this logical basis; for there are few mistakes more mischievous than the supposition that a theory supported by certain evidence derives from that evidence a probability equal to that of the evidence itself. It is absolutely certain that the one planet we know is inhabited; but it by no means follows certainly that planets like the earth support life, still less that planets unlike the earth do so, and least of all that every planet is now the abode of life.

A higher degree of probability in favour of the
theory that there are many inhabited worlds arises
from a consideration of the *manner* in which life
exists on the earth. If one could judge of a *purpose*
(according to our way of thinking) in all that is going
on around us, our earth might teach us to regard the
support of life as Nature's great purpose. Earth,
water, and air, alike teem with life. No peculiarities
of climate seem able to banish life. As I have said,
elsewhere, ' in the bitter cold within the Arctic regions,
with their strange alternations of long summer days
and long winter nights, their frozen seas, perennial ice,
and scanty vegetation, life flourishes in a hundred dif-
ferent forms. On the other hand, the torrid zone,
with its blazing heat, its long-continued droughts, its
strange absence of true seasonal changes, and its trying
alternations of oppressive calms and fiercely raging
hurricanes, nourishes even more numerous and varied
forms of life than the great temperate zones. Around
mountain summits as in the depths of the most secluded
valleys, in mid-ocean as in the arid desert, in the air
as beneath the surface of the earth, we find a myriad
forms of life.' Nor is the scene changed when, with
the mind's eye, we contemplate the earth during past
ages of her history, even to the most remote stage of
her existence as a planet fit to be the abode of life.
Whenever there was life at all, there was abundant
life. For, though no traces remain of a million forms
of life which co-existed with the few forms recognised
as belonging to this or that geologic era, yet we can

infer from the forms of which traces remain that others
must have been present which have left no trace of
their existence. The skeletons of mighty carnivora
assure us that multitudes of creatures existed on which
those monsters fed. The great sea creatures whose
remains have been found, attest the existence of many
races of small fish. The mighty Pterodactyl did not
range through desert aërial regions, for he could exist
only where many orders of aërial creatures also existed.
Of minute creatures inhabiting the water we have re-
cords in the strata formed as generation after genera-
tion sank to the sea bottom after death, whereas the
correspondingly minute inhabitants of the land and of
the air have left no trace of their existence ; yet we
can feel no reasonable doubt that in every geologic
age, forms of minute life were as rich in air and on the
land as in the sea, or as they now are in all three. Of
insect life all but a few traces have passed away,
though occasionally, by some rare accident, even so
delicate a structure as a butterfly's wing has left its
record, not only attesting the existence of hosts of
insects, but showing that delicate flowers with all the
charms of sweet perfume and variegated colour, existed
in those times as in ours. It is no mere speculation,
then, but the direct and unquestionable teaching of
geology, that throughout the whole time represented
by the fossiliferous rocks, life of all kinds was most
abundant on our earth.

And while we thus recognise throughout our earth's
history as a planet, Nature's apparent purpose of pro-

viding infinitely varied forms of life at all times and under the most varied conditions, we also perceive that Nature possesses a power of modifying the different types in accordance with the varying conditions under which they subsist. Without entering here into the vexed question of the actual extent to which the principle of selection operates, we must admit that it does operate largely, and that it must necessarily cause gradual change of every type of living creature towards the most suitable form. This particular operation of Nature must certainly be regarded as an apparent carrying out of the purpose attributed to her,—by our manner of speaking when we say that Nature's one great object is the support of life. If types were unchangeable, life would come to an end upon a globe whose condition is not only not unchangeable, but changes largely in the course of long periods of time. But types of life change, or can change when required, at least as quickly as the surrounding conditions—save in the case of certain catastrophes, which however never affect any considerable proportion of the earth's surface.

Nor is it easy to assign any limits to this power of adaptation, though we can scarcely doubt that limits exist. The earth may so change in the course of hundreds of thousands of years to come that none of the chief forms of life, animal or vegetable, at present existing, could live even for a single year under the changed conditions of those distant times, while yet the descendants of creatures now living (including man)

may be as well fitted to the circumstances around them
as the most favoured races of our own time.   Still
there must be a limit beyond which the change of the
earth's condition, whether through the cooling of her
own globe or the diminution of the sun's heat, will be
such that no conceivable modification of the types of
life now existing could render life possible.   It must
not be forgotten that Nature's power of adaptation is
known to be finite in many cases, and therefore must
be presumed to be finite in all cases.   The very pro-
cess of selection by which adaptation is secured implies
the continual failure of preceding adaptations.   The
struggle for life involves the repeated victory of death.
The individuals which perish in the struggle (that is,
which perish untimely) far outnumber those which
survive.   And what is true of individuals is true of
types.   Nature is as wasteful of types as she is of
life—

> So careful of the type ; but no,
> From scarped cliff and quarried stone
> She cries, ' a thousand types are gone ;
> I care for nothing, *all* shall go.'

This is, in truth, what we must believe, if, reasoning
by analogy, we pass but one step higher in the scheme
of creation.   We know that Nature, wasteful of indi-
vidual life, is equally wasteful of types of life.   Must
we not infer that she is no less wasteful of those aggre-
gations of types which constitute the populations of
worlds ?   Watching her operations a few brief minutes
we might (setting experience aside) suppose her care-
ful of individual life.   Watching during a few genera-

tions, we should pronounce her careful of the type, though careless of individual life. But we perceive, when we extend the range of time through which we look, that she is careless no less of the type than of life. Why should this extension of the range of view be the last we should permit ourselves? If we pronounce Nature careful of the planetary populations, though careless of the types of life which make up such populations, we are simply declining to take a further step in the course pointed out for us by the teachings of analogy.

Let us go over the ground afresh. Individual creatures, even the most favoured, perish after a time, though the balance may long oscillate between life and death. Weak, at first, each creature which is to live grows at length to its full strength, not without vicissitudes which threaten its existence. As its life progresses the struggle continues. At one time the causes tending to decay seem to prevail awhile; at another, those which restore the vital powers. Disease is resisted again and again; at first easily, gradually with greater difficulty, until at length death wins the day. So it is with types or orders of living creatures. A favoured type, weak at first, begins after awhile to thrive, and eventually attains its fullest development. But from time to time the type is threatened by dangers. Surrounding conditions become less favourable. It ceases to thrive, or, perhaps, passes through successive alternations of decay and restoration. At length the time comes when the struggle for existence can manifestly have but one end; and then, though

the type may linger long before it actually disappears,
its disappearance is only a question of time.   Now, it
is true that each type thus flourishing for awhile springs
from other types which have disappeared.   The favoured
types of our age are but varieties of past types.   Yet
this does not show that types will continue to suc-
ceed each other in endless succession.   For, if we
consider the matter rightly, we perceive that the
analogue of this circumstance is, in the case of indi-
vidual life, the succession of living creatures generation
after generation.   And as we know that each family,
however large, dies out in the long run unless recruited
from without, so we are to infer that the various types
peopling this earth, since they cannot be recruited
from without, must at length die out, though to our
conceptions the time necessary for this process may
appear infinite.

To the student of science who recognises the true
meaning of the doctrine that force can be neither
annihilated nor created, it will indeed appear manifest
that life must eventually perish from the face of the
earth; for he perceives that the earth possesses now a
certain fund or store of force in her inherent heat,
which is continually though slowly passing away.   The
sun also, which is a store-house whence certain forms
of force are distributed to the earth, has only a finite
amount of energy (though probably the inhabitants of
the earth are less directly concerned in this than in the
finiteness of terrestrial forces).   Life of all kinds on
the earth depends on both these stores of force, and

when either store is exhausted life must disappear from the earth. But each store is in its nature limited, and must one day, therefore, be exhausted.

We have also only to consider that life on the earth necessarily had a beginning to infer that it must necessarily have an end. Clearest evidence shows how our earth was once ' a fluid haze of light,' and how for countless æons afterwards her globe was instinct with fiery heat, amidst which no form of life could be conceived to exist, after the manner of life known to us, though the germs of life may have been present ' in the midst of the fire.' Then followed ages in which the earth's glowing crust was drenched by showers of muriatic, nitric, and sulphuric acid, not only intensely hot, but fiercely burning through their chemical activity. Only after periods infinite to our conceptions could life such as we know it, or even in the remotest degree like what is now known to us, have begun to exist upon the earth.

The reader, doubtless, perceives whither these considerations tend, and how they bear in an especial manner on the opinion we are to form respecting the two planets Mars and Jupiter. We see our earth passing through a vast period, from its first existence as a separate member of the solar system, to the time when life appeared upon its surface : then began a comparatively short period, now in progress, during which the earth has been and will be the abode of life ; and after that must follow a period infinite to our conceptions when the cold and inert globe of the

earth will circle as lifelessly round the sun as the moon
now does.   We may, if we please, infer this from
analogy, seeing that the duration of life is always in-
finitely small by comparison with the duration of the
region where life appears; so that, by analogy, the
duration of life on the earth would be infinitely short
compared with the duration of the earth itself.   But
we are brought to the same conclusion independently
of analogy, perceiving that the fire of the earth's
youth and the deathly cold of her old age must alike
be infinite in duration compared with her period of
vital life-preserving warmth.   And what is true of the
earth is true of every member of the solar system,
major planet, minor planet, asteroid, or satellite;
probably of every orb in space, from the minutest me-
teorite, to suns exceeding our sun a thousandfold in
volume.

If we had any reason to suppose that all the
planets sprang simultaneously into being, that each
stage of each planet's existence synchronized with the
same stage for every other planet, and that life ap-
peared and disappeared at corresponding stages in the
existence of every planet, we should be compelled to
accept the theory that at this moment every planet is
the abode of life.   Not only, however, have we no reason
to suppose that any one of these conditions exists (and
not one but *all* these conditions must exist before that
theory can be accepted), but we have the strongest
possible evidence, short of actual demonstration, that
the births of the different planets occurred at widely

remote periods, and that the several stages of the different planets' growth differed enormously in duration ; while analogy, the only available evidence on the third point, assures us that little resemblance can be supposed to exist between the conditions and requirements of life in different members of the solar system.

On any reasonable hypothesis of the evolution of the solar system, the eight primary planets must have begun to exist as independent bodies at very different periods. If we adopt Laplace's theory of the gradual contraction of a mighty nebula, then we should infer that the planets were formed in the order of their distances from the sun, the remoter planets being those formed first. And according to the conditions of Laplace's hypothesis, the interval separating the formation of one planet from that of its next neighbour on either side must have been of enormous duration. If we prefer the theory of the gradual growth of each planet by processes of accretion, we should infer perhaps that the larger planets took longest in growing to maturity, or preferably that (according to the doctrine of probabilities) a process which for the whole system must have been of inconceivably enormous length, and in which the formation of one planet was in no sort connected with the formation of any other, could not have resulted in bringing any two planets to maturity at the same or nearly the same time, save by so improbable a combination of fortuitous circumstances as may justly be considered impossible. If we consider that the solar system was evolved by a combina-

tion of both processes (the most probable theory of the three in my opinion), we must still conclude that the epochs of the formation of the different planets were separated by time-intervals so enormous that the duration of life upon our earth is, by comparison, as a mere second compared with a thousand years.

Again, if we compare any two members of the solar system, except perhaps Venus and the Earth, we cannot doubt that the duration of any given stage of the existence of one must be very different from that of the corresponding stage in the other. If we compare, for instance, Mars with the Earth, or the Earth with Jupiter, and still more, if we compare Mars with Jupiter, we cannot doubt that the smaller orb of each pair must pass much more rapidly through the different stages of its existence than the larger. The laws of physics assure us of this, apart from all evidence afforded by actual observation; but the results of observation confirm the theoretical conclusions deduced from physical laws. We cannot, indeed, study Mars in such sort as to ascertain his actual physical condition. We know that his surface is divided into lands' and seas, and that he possesses an atmosphere; we know that the vapour of water is at times present in this atmosphere·; we can see that snows gather over his polar regions in winter and diminish in summer : but we cannot certainly determine whether his oceans are like our own or for the most part frozen; the whitish light which spreads at times over land or sea may be due to clouds or to light snow-falls, for aught that ob-

servation shows us; the atmosphere may be as dense as our own or exceedingly rare; the polar regions of the planet may resemble the earth's polar regions, or may be whitened by snows relatively quite insignificant in quantity. In fine, so far as observation extends, the physical condition of Mars may closely resemble that of the earth, or be utterly dissimilar. But we have indirect observational means of determining the probable condition of a planet smaller than the earth, and presumably older—that is, at a later stage of its existence. For the moon is such a planet, and the telescope shows us that the moon in her decrepitude is oceanless, and is either wholly without atmosphere or possesses an atmosphere of exceeding tenuity. Hence we infer that Mars, which, as an exterior planet and much smaller than the earth, is probably at a far later stage of its existence, has passed far on its way towards the same state of decrepitude as the moon. As to Jupiter, though he is so much farther from us than Mars, we have direct observational evidence, because of the vast scale on which all the processes in progress on his mighty globe are taking place. We see that his whole surface is enwrapped in cloud layers of enormous depth, and undergoing changes which imply an intense activity (or, in other words, an intense heat) throughout his whole mass. We recognise in the planet's appearance the signs of as near an approach to the conditions of the earth when as yet the greater part of her mass was vaporous, as is consistent with the

vast difference necessarily existing between two orbs containing such unequal quantities of matter.

Mars, on the one hand, differs from the earth in being a far older planet,—*probably*, as respects the actual time which has elapsed since the planet was formed, and *certainly*, as respects the stage of its career which it has now reached. Jupiter, on the other hand, differs from the earth in being a far younger planet, not in years perhaps, but in condition. As to the actual age of Jupiter we cannot form so probable an opinion as in the case of Mars. Mars being an exterior planet, must have *begun* to be formed long before the earth, and, being a much smaller planet, was probably a shorter time in attaining his mature growth : on both accounts, therefore, he would be much older than the earth in years; while, as we have seen, his relative smallness would cause the successive stages of his career subsequent to his existence as an independent and mature planet to be much shorter. Jupiter, being exterior to Mars, presumably began to be formed millions of centuries before that planet, but his bulk and mass so enormously exceed those of Mars that his growth must have required a far longer time ; so that it is not at all certain that even in point of years Jupiter (dating from his maturity) may not be the youngest member of the solar system. But even if not, it is practically certain that, as regards development, Jupiter is far younger than any member of the solar system, save perhaps his brother giant Saturn, whose greater antiquity and inferior mass (both suggesting a later

stage of development) may have been counterbalanced by a comparative sluggishness of growth in the outer parts of the solar domain.

It is manifest from observed facts, in the case of Jupiter, that he is as yet far removed from the life-bearing stage of planetary existence, and theoretical considerations point to the same conclusion. In the case of Mars, theoretical considerations render it extremely probable that he has long since passed the life-bearing stage, and observed facts, though they do not afford strong evidence in favour of this conclusion, suggest nothing which, rightly considered, is opposed to it. It is true that, as we have shown in former essays on this planet, Mars presents many features of resemblance to our earth. This planet rotates in a period not differing much from our day; his year does not exceed ours so greatly as to suggest relations unpleasantly affecting living creatures; it has been shown that there are oceans on Mars, though it is not quite so clear that they are not for the most part frozen; he has an atmosphere, and the vapour of water is at times present in that atmosphere as in ours; clouds form there; snow falls, and perhaps rain from time to time; ice and snow gather at the poles in winter, and are partially melted in summer; the land surface must necessarily be uneven, seeing that the very existence of continents and oceans implies that once, at any rate, the globe of Mars was subjected to forces resembling those which have produced the irregularities of the earth's surface; glacial action must still be going on

there, even if there is no rainfall, and therefore no denuding action corresponding to that which results from the fall of rain on our terrestrial continents.   But it is a mistake (and a mistake too commonly made) to suppose that the continuance of those natural processes which are advantageous to living creatures, implies the existence of such creatures.   The assumption is that the beneficent processes of nature are never wasted according to our conceptions.   Yet we see over and over again in nature not merely what resembles waste, what in fact *is* waste according to our ideas, but an enormous excess of wasted over utilized processes.   The sun pours forth on all sides the supplies of light and heat which, where received as on our earth, sustain vegetable and animal life ; but the portion received by our earth is less than the 2000· millionth, the portion received by all the planets less than the 230 millionth part, of the total force thus continually expended. And this is typical of nature's operations everywhere. The earth on which we live illustrates the truth as clearly as the sun.   We are apt to say that it teems with life, forgetting that the region occupied by living creatures of all orders is a mere shell, while the whole interior mass of the earth, far larger in volume, and undergoing far more active processes of change—teeming in fact with energy—contains no living creature, or at least can only be supposed to contain living creatures by imagining conditions of life utterly different from those we are familiar with.

The mere continuance, therefore, on Mars of pro-

cesses which on the earth we associate with the exis-
tence of life, in reality proves nothing as to the con-
tinued existence of life on Mars.   The surface of the
moon, for example, must undergo disturbances,—mighty
throes, as the great wave of sun-distributed heat circles
round her orb once in each lunation,—yet few suppose
that there is life, or has been for untold ages, on the
once teeming surface of our companion planet.   The
formation of Mars as a planet must so long have pre-
ceded that of our earth, his original heat must have
been so much less, his small globe must have parted
with such heat as it once had so much more rapidly,
Mars lies so much farther from the sun than our earth
does, his atmosphere is so much rarer, his supply of
water (the temperature-conserving element) is rela-
tively as well as absolutely so much smaller, that his
surface must be utterly unfit to support life in the
remotest degree resembling the forms of life known on
earth (save, of course, those lower forms which from
the outset we have left out of consideration).   Yet at
one time, a period infinitely remote according to our
conceptions of time, the globe of Mars must have
resembled our earth's in warmth, and in being dis-
turbed by the internal forces which cause that con-
tinual remodelling of a planet's surface without which
life must soon pass away.   Again, in that remote
period the sun himself was appreciably younger; for
we must remember that although, measured by ordi-
nary time-intervals, the sun seems to give forth an
unvarying supply of heat day by day, a real process of

exhaustion is in progress *there* also. At one time there must have existed on Mars as near an approach to the present condition of our earth, or rather to her general condition during this life-supporting era of her existence, as is consistent with the difference in the surface gravity of the planets, and with other differences inherent as it were in their nature. Since Mars must also have passed through the fiery stage of planetary life and through that intermediate period when, as it would seem, life springs spontaneously into being under the operation of natural laws not as yet understood by us, we cannot doubt that when his globe was thus fit for the support of life, life existed upon it. Thus for a season,—enormously long compared with our ordinary time-measures, but very short compared with the life-supporting era of our earth's career,— Mars was a world like our own, filled with various forms of life. Doubtless, these forms changed as the conditions around them changed, advancing or retrograding as the conditions were favourable or the reverse, perhaps developing into forms corresponding to the various races of men in possession of reasoning powers, but possibly only attaining to the lower attributes of consciousness when the development of life on Mars was at its highest, thenceforth passing by slow degrees into lower types as the old age of Mars approached, and finally perishing as cold and death seized the planet for their prey.

In the case of Jupiter, we are guided by observed facts to the conclusion that ages must elapse before

life can be possible. Theory tells us that this
mighty planet, exceeding the earth three hundred
times in mass, and containing five-sevenths of the mass
of the whole system of bodies travelling around the sun,
must still retain a large portion of its original heat,
even if we suppose its giant orb took no longer in
fashioning than the small globe of our earth. Theory
tells us moreover that so vast a globe could not possi-
bly have so small a density (less than one-fourth the
earth's) under the mighty compressing force of its own
gravity, unless some still more potent cause were at
work to resist that tremendous compression—and this
force can be looked for nowhere but in the intense
heat of the planet's whole mass. But observation
shows us also that Jupiter is thus heated. For we see
that the planet is surrounded by great cloud-belts such
as our own sun would be incompetent to raise,—far
more so the small sun which would be seen in the skies
of Jupiter if already a firmament had been set ' in the
midst of the waters.' We see that these belts undergo
marvellous changes of shape and colour, implying the
action of exceedingly energetic forces. We know from
observation that the region in which the cloud-bands
form is exceedingly deep, even if the innermost region
to which the telescope penetrates is the true surface of
the planet—while there is reason for doubting whether
there may not be cloud-layer within cloud-layer, to a
depth of many thousand miles,—or even whether the
planet has any real surface at all. And, knowing from
the study of the earth's crust that for long ages the

whole mass of our globe was in a state of fiery heat, while a yet longer period preceded this when the earth's globe was vaporous, we infer from analogy that Jupiter is passing, though far more slowly, through stages of his existence corresponding with terrestrial eras long anterior to the appearance of life upon the scene.

We must, then, in the case of Jupiter, look to a far distant future for the period of the planet's existence as a life-sustainer. The intense heat of the planet must in course of time be gradually radiated away into space, until at length the time will come when life will be possible. Then, doubtless, will follow a period (far longer than the life-sustaining portion of the earth's existence) during which Jupiter will in his turn be the abode of life. It may be that before then the sun will have lost so large a proportion of heat that life on Jupiter will be mainly sustained by the planet's inherent heat. But more probably the changes in the sun's heat take place far more slowly than the changes in the condition of any planet, even the largest. Possibly, even, the epoch when Jupiter will be a fit abode for life, will be so remote that the sun's fires will have been recruited by the indrawing of the interior family of planets. For it must be remembered that the periods we have to deal with in considering the cooling of such an orb as Jupiter are so enormous, that not merely the ordinary time-measures, but even the vast periods dealt with by geologists must be insignificant by comparison. Yonder is Jupiter still enwrapped in clouds of vapour raised by his internal heat, still seething, as it were, in

his primeval fires, though the earth has passed through all the first stages of her existence, and has even long since passed the time of her maturity as a life-sustaining globe. It is no mere fancy to say that all the eras of Jupiter's existence must be far longer than the corresponding terrestrial eras, since we actually see Jupiter in that early stage of his existence and know that the earth has passed through many stages towards the final eras of decay and death. It is indeed impossible to form any opinion as to the probable condition of the sun or of the solar system when Jupiter shall become fit to support life, seeing that, for aught we know, far higher cycles than those measured by the planetary motions may have passed ere that time arrives. The sun may not be a solitary star but a member of a star-system, and before Jupiter has cooled down to the life-sustaining condition, the sun's relation to other suns of his own system may have altered materially, although no perceptible changes have occurred during the relatively minute period (a trifle of four thousand years or so) since astronomy began.

And as, in considering the case of Mars, we suggested the possibility that, owing to the relative shortness of that planet's life-sustaining era, the development of the higher forms of life may have been less complete than on our earth thus far, (still less than the development of those forms on the earth in coming ages), so we may well believe that during the long period of Jupiter's existence as a life-supporting planet, creatures far higher in the scale of being than any that have in-

habited, or may hereafter inhabit, the earth, will be brought into existence. As the rule of nature on earth has been to advance from simple to more complex forms, from lower types to higher, so (following the argument from analogy) we must suppose the law of nature to be elsewhere. And time being a necessary element in any process of natural development, it follows that where nature is allowed a longer time to operate, higher forms, nobler types, will be developed. If this be so, then in Jupiter, the prince of planets, higher forms of animated conscious being will doubtless be developed than in any other planet. We need not indeed point out that the supposition on which this conclusion rests is merely speculative, and that now, when the laws of natural development have so recently begun to be recognised and are still so imperfectly known, the argument from analogy is (in this particular case) necessarily weak. Nevertheless, analogy points in the direction we have indicated, and it is well to look outwards and onwards in that direction, even though the objects within the field of view are too remote for us to perceive their real forms.

But, limiting our conclusions to those which may be justly inferred from known facts, let us inquire how the subject of life in other worlds presents itself when dealt with according to the relations above considered.

It is manifest at once that whether our new ideas respecting the present condition of Mars or Jupiter be correct or not, the general argument deducible from the analogy of our own earth remains unaffected. If Mars and Jupiter be at this moment inhabited by living

creatures, it can only be because these orbs happen to be passing through the life-supporting period of their existence. We have shown that there is strong reason for believing this not to be the case ; but if it is the case, this can only be regarded as a strange chance. For we have learned from the study of our earth, that the life-supporting era of a planet is short compared with the duration of the planet's existence. It follows that any time selected at random in the history of a planet is far more likely to belong to one or other of the two lifeless eras, one preceding, the other following the life-supporting era, than to belong to this short era itself. And this present time *is* time selected at random with reference to any other orb in the universe than our own earth. We are so apt to measure all the operations of nature by our own conceptions of them, as well in space as in time, that as the solar system presents itself (even now) as the centre of the universe, so this present time, the era of our own life, or of our nation's life, or of the life of man, or of the existence of organic beings on the earth, or (passing yet a grade higher) the era of our earth's existence as a planet, presents itself to us as the central era of *all* time. But what has been shown to be false with respect to space is equally false with respect to time. Men of old thought that the petty region in which they lived was the centre of the universe. After this was shown to be false by Copernicus, Kepler, and Newton, men clung in turn to the conception that the solar system is central within the universe. The elder Herschel showed that this conception also is false. Even he, however,

assigned to the sun a position whence the galaxy might be measured. But it begins to be recognised that this is not so. Nay, not only is the sun no suitable centre whence to measure the stellar system, but the stellar system is for us immeasurable. The galaxy has no centre and no limits; or rather we may say of it what Blaise Pascal said of the universe of space—its centre is everywhere and its circumference nowhere. The whole progress of modern science tends to show that we must similarly extend our estimate of time. In former ages each generation was apt to regard its own era as critical in the earth's history, that is, according to their ideas, in the history of the universe itself. Gradually men perceived that no generation of men, no nation, no group of nations, occupies a critical or central position in the history of even the human race upon earth, far less in the history of organic life. We may now pass a step higher, and, contemplating the infinity of time, admit that the whole duration of this earth's existence is but as a single pulsation in the mighty life of the universe. Nay, the duration of the solar system is scarcely more. Countless other such systems have passed through all their stages, and have died out, untold ages before the sun and his family began to be formed out of their mighty nebula; countless others will come into being after the life has departed from our system. Nor need we stop at solar systems, since within the infinite universe, without beginning and without end, not suns only, but systems of suns, galaxies of such systems, to higher and higher orders

endlessly, have long since passed through all the stages
of their existence as systems, or have all those stages
yet to pass through. In the presence of time-intervals
thus seen to be at once infinitely great and infinitely
little—infinitely great compared with the duration of
our earth, infinitely little by comparison with the
eternities amidst which they are lost—what reason can
we have when viewing any orb in space from our little
earth, for saying *now* is the time when that orb is,
like our earth, the abode of life? Why should life on
that orb synchronize with life on the earth? Are not,
on the contrary, the chances infinitely great against
such a coincidence? If, as Helmholtz has well said,
the duration of life on our earth is but the minutest
'ripple in the infinite ocean of time,' and the duration
of life on any other planet of like minuteness, what
reason can we have for supposing that those remote,
minute, and no way associated waves of life must needs
be abreast of each other on the infinite ocean whose
surface they scarcely ripple?

But let us consider the consequences to which we
are thus led. Apart from theoretical considerations or
observed facts, it is antecedently improbable that any
planet selected at random, whether planet of our own
system, or planet attending on another sun than ours,
is at this present time the abode of life. The degree
of improbability corresponds to the proportion between
the duration of life on a planet, and the duration of
the planet's independent existence. We may compare
this proportion to that existing between the average

lifetime of a man and the duration of the human race.
If one person were to select at random the period of a
man's life, whether in historic, prehistoric, or future
time, and another were to select an epoch equally at
random, save only that it fell *somewhere* within the
period of the duration of the human race, we know how
exceedingly minute would be the probability that the
epoch selected by the second person would fall within
the period selected by the first. Correspondingly
minute is the *à priori* probability that at this present
epoch any planet selected at random is the abode of life.
This is not a mere speculation, but an absolute cer-
tainty, if we admit as certain the fact, which few now
question, that the period during which organic exist-
ence is possible on any planet is altogether minute com-
pared with the duration of that planet's existence.

The same relation is probably true when we pass to
higher systems. Regarding the suns we call 'the stars'
as members of a sidereal system of unknown extent
(one of innumerable systems of the same order),
the chance that any sun selected at random is, like
our own sun at the present time, attended by a
planetary system in one member of which at least life
exists, is exceedingly small, if, as is probable, the life-
supporting era of a solar system's existence is very short
compared with the independent existence of the system.
If the disproportion is of the same order as in the case
of a single planet, the probability is of the same order
of minuteness. In other words, if we select any star
at random, it is as unlikely that the system attending

on that sun is at present in the life-bearing stage as a
system, as it is that any planet selected at random is
at present in the life-bearing stage as a planet. This
conclusion, indeed, may be regarded as scarcely less
certain than the former, seeing that we as little doubt
the relative vastness of the periods of our sun's exist-
ence before and after his existence as a supporter of life,
as we doubt the relative vastness of the periods before
and after the life-supporting era of any given planet.
There is, however, one element of doubt in the case of
the star. The very fact of the star's existence, as a steady
source of light and heat implies that the star is in a
stage resembling that through which our own sun is now
passing. It may be for instance that the prior stages
of solar life are indicated by some degree of nebulosity,
and the later stages by irregular variations, or by such
rapid dying out in brightness as has been observed in
many stars. Yet a sun must be very nebulous indeed—
that is, must be at a very early stage in its history—
for astronomers to be able to detect its nebulosity ; and
again, a sun must long have ceased to be a life-sup-
porter, before any signs of decadence measurable at our
remote station, and with our insignificant available
time-intervals for comparison, are manifested.

As to higher orders than systems of suns we cannot
speculate, because we have no means of determining
the nature of such orders. For instance the arrange-
ment and motions of the only system of suns we know
of, the galaxy, are utterly unlike the arrangement and
motions of the only system of planets we know of.

Quite possibly systems of sun-systems are unlike either
galaxies or solar systems in arrangement and motions.
But if, by some wonderful extension of our perceptive
powers, we could recognise the countless millions of
systems of galaxies doubtless existing in infinite space,
without however being able to ascertain whether the
stage through which any one of those systems was
passing corresponded to the stage through which our
galaxy is at present passing, the probability of life
existing anywhere within the limits of a galaxy so
selected at random would be of the same order as the
probability that life exists either in a planet taken at
random, or in a solar system taken at random.   For
though the number of the suns is enormously increased,
and still more the number of subordinate orbs like
planets (*in posse* or *in esse*), the magnitude of the
time-intervals concerned is correspondingly increased.
One chance out of a thousand is as good as a thousand
chances out of a million, or as a million out of a
thousand millions.   Whether we turn our thoughts to
planet, sun, or galaxy, the law of nature (recognised as
universal within the domain as yet examined), that the
duration of life in the individual is indefinitely short
compared with the duration of the type to which the in-
dividual belongs, assures us, or at least renders it highly
probable, that in any member of any of these orders
taken at random, *it is more probable that life is want-
ing than that life exists at this present time.*   Neverthe-
less it is at least as probable that *every member of every
order—planet, sun, galaxy, and so onward to higher*

and higher orders endlessly—has been, is now, or will
hereafter be, life-supporting ' after its kind.'

In what degree life-supporting worlds, or suns, or
systems are at this or any other epoch surpassed in
number by those which as yet fulfil no such functions
or have long since ceased to fulfil them, it would only
be possible to pronounce if we could determine the
average degree in which the life-sustaining era of given
orbs or systems is surpassed in length by the preceding
and following stages. The life-sustaining orbs or systems
may be surpassed many thousandfold or many million-
fold in number by those as yet lifeless or long since
dead, or the disproportion may be much less or much
greater. As yet we only know that it must be very
great indeed.

But at first sight the views here advanced may
appear as repugnant to our ordinary ideas as Whewell's
belief that perhaps our earth is the only inhabited orb
in the universe. Millions of uninhabited worlds for
each orb which sustains life! surely that implies
incredible waste! If not waste of matter, since
according to the theory every orb sustains life in its
turn, yet still a fearful waste of time. To this it may
be replied, first that we must take facts as we find
them. And secondly, whether space or matter or
time or energy appear to be wasted, we must consider
that, after all, space and matter and time and energy are
necessarily infinite, so that the portion utilized (accord-
ing to our conceptions) being a finite portion of the
infinite is itself also infinite. Speaking, however, of

the subject we are upon, if one only of each million of the orbs in the universe is inhabited, the number of inhabited orbs is nevertheless infinite. Moreover, it must be remembered that our knowledge is far too imperfect for us to be able to assert confidently that space, time, matter, and force, though not utilized according to our conceptions, are therefore necessarily wasted. To the ignorant savage, grain which is planted in a field instead of being used for food, seems wasted, the wide field seems wasted, the time wasted during which the grain is growing and ripening into harvest; but wiser men know that what looks like waste is in reality economy. In like manner the sun's rays poured on all sides into space so that his circling family receives but the 230 millionth portion, seem, to our imperfect conceptions, almost wholly wasted; but, if our knowledge were increased, we should perhaps form a far different opinion. So it may well be with the questions which perplex us when we contemplate the short duration of the life-sustaining condition of each world and sun and galaxy compared with the whole existence of these several orders. The arrangement which seems so wasteful of space and time and matter and force, may in reality involve the most perfect possible use and employment of every portion of space, every instant of time, every particle of matter, every form of force.

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THE PLANETS PUT IN LEVERRIER'S BALANCE.

LEVERRIER has recently completed the noblest work in pure astronomy which this age has seen.   Five-and-thirty years ago he began to weigh the planets of the solar system in the balance of mathematical analysis. 'To-day,' said he, addressing the Academy of Sciences at Paris, on December 21 last, 'I have the honour to present a paper completing the *ensemble* of work the first piece of which goes back to September 16, 1839.'   At that time he had only seven leading planets to deal with ; it affords some idea of the nature of his work that the discovery of the eighth planet, Neptune, was a mere incident in the progress of his labours. Perplexed by peculiarities in the motion of one particular planet of the set he had undertaken to weigh, Leverrier quietly undertook to calculate the cause of those peculiarities, and so found Neptune.   It was a matter of small account that another great mathematician almost simultaneously accomplished the same task.   With Adams the discovery of the unknown planet was the ultimate object of inquiry ; with Leverrier it was a mere step in a long series of investigations.   To the outside world indeed it was the achievement of all others most deserving of notice in Leverrier's work, just as the discovery of Uranus by Sir W. Herschel attracted attention which labours altogether more important both in their nature and in their

results had failed to secure. But Leverrier himself can hardly have so regarded the discovery of Neptune. For him, its chief interest must have resided in the confirmation of his method of procedure afforded by the discovery of a planet through the careful study of perturbations due to that planet's attraction. Such confirmation was afforded at other steps of the work. In fact the whole series of Leverrier's labours affords perhaps the noblest illustration of the value of deduction guided by and suggesting observations since Newton's *Principia* first proved the superiority of that method over *mere* induction.[1]

I propose to give such a sketch of Leverrier's method and results as would alone be suited to these pages. It need hardly be said, perhaps, that his work

[1] According to Bacon, science was to be advanced by making great collections of observations and classifying them—sorting and sifting until the grains of truth were winnowed out. No great discovery has ever been effected in this manner. The real use of observation and experiment has been found in their application to test the deductions from theories formed long before materials sufficient for Bacon's inductive method had been gathered. The question is one of fact. Theoretically, Bacon's method is perfect; it has hitherto failed in practice. Take any of the great discoveries of science, and it will be found that observations and experiments merely gathered together had no part in leading to the discovery; but that observations and experiments suggested by the deductions from theory were all-important. The moon might have been observed at Greenwich for all time without the observations leading to the discovery of gravitation. But Newton's deductions from the theory (when as yet the theory was but a guess) at once showed what observation might do; and it was by observation so made that the theory was established. In spectrum analysis a perfect heap of experiments had been collected without any useful results. Kirchhoff is led by a single observation to think of a theory, deduces certain consequences, tests these by three experiments, and the great discovery is to all intents and purposes effected.

is essentially mathematical—nay, his method, though not belonging to the very highest developments of modern mathematics, requires (even to be understood) a higher degree of mathematical skill than would be implied by mere familiarity with more recent methods in mathematics. Yet it is possible to exhibit the general principles and the results of Leverrier's work in a manner which everyone can understand.

. In the solar system, we see first a mighty central ruler, whose mass so enormously exceeds that of all the planets taken together, that he is capable of swaying their motion without being himself disturbed. He is not indeed quite fixed. Whatever force he exerts on any planet, precisely that same force the planet exerts on him ; but then he is so massive that the pull which compels the planet to circle around the sun scarcely displaces him at all. 'If he pulls the planets,' says Sir John Herschel, 'they pull him and each other; but such family struggles affect him but little. *They amuse them,*' he proceeds quaintly, ' *but don't disturb him.* As all the gods in the ancient mythology hung dangling from and tugging at the golden chain which linked them to the throne of Jove, but without power to draw him from his seat, so, if all the planets were in one straight line and exerting their joint attractions, the sun—leaning a little back as it were to resist their force—would not be disturbed by a space equal to his own radius ; and the fixed centre, or as an engineer would call it, the centre of gravity of our system, would still lie far within the sun's globe.'

To give clearness to our conceptions, let the mass
of the sun be compared with that of all the other
planets taken together. If we take the earth's mass
as one thousand, then the mass of the eight chief
planets of the solar system is represented by about
four hundred and twenty-two thousand, and the sun's
mass by three hundred and fifteen millions. Thus
the sun's mass exceeds that of the whole system
nearly seven hundred and fifty times; for in such a
computation the combined mass of all such bodies
as the asteroids, moons, meteors, &c., counts for
nothing.

We see, then, that the movements of the eight
planets must necessarily be determined in the main by
the sun's attractive energy. What can even Jupiter,
the mightiest of all the planets, do to disturb his giant
neighbour Saturn from the path on which the sun, a
giant so far mightier than either, would, by his attrac-
tive energy, compel the ringed planet to travel? The
sun is more than a thousand times more massive than
Jupiter, and though Jupiter when between the sun and
Saturn is at but one-half the sun's distance, yet this
nearness only quadruples the relatively small power of
Jupiter, and leaves the sun's force on Saturn still two
hundred and fifty times greater. Besides, Jupiter is
only from time to time placed in this favourable posi-
tion. Half the time he is even farther from Saturn
than the sun is, and thus exerts less than a thousandth
part of the sun's influence. And it need hardly be
said that, if Jupiter is thus ineffective in disturbing a

neighbouring planet, every other planet is still weaker
to disturb its neighbours. Our earth, for instance,
with a mass barely equal to one three-hundred-and-
fifteen-thousandth part of the sun's, has but small
power to disturb her nearest neighbours, Mars and
Venus, from that steady motion in their sun-ruled
orbits which they would have if the earth did not exist.
Venus is still weaker in disturbing the earth and Mer-
cury, her neighbours; Mars weaker still; and Mercury
weakest of all. Nor does the gradual diminution of
the planetary distances as we draw nearer to the sun
at all increase the relative disturbing power of the
different planets. It might seem that the contrary
should be the case. For instance, the other day, when
Venus was in transit she was but about twenty-four
millions of miles from us, and it might seem that
Venus must then have disturbed the earth, and the
earth Venus, very much more effectively (in proportion
to their mass) than Jupiter can disturb Saturn or
Saturn Jupiter, seeing that these planets never
approach within three hundred and fifty millions of
miles from each other. But in reality, the effect of
proximity in such cases is counterbalanced by the
much greater velocity with which the nearer planets
travel. It would be easy to make an exact com-
parison, but the calculation would be unsuited to these
pages. Let it suffice to say that throughout the whole
of the solar system there is no disturbance greater than
that resulting from the mutual attraction of Jupiter
and Saturn; and how small this attraction is, com-

pared with the sun's influence on either planet, we have already seen.

The sun being thus placed as supreme ruler over the motions of the planets, their motions, starting from any given moment as a beginning, are in the main those due to solar influences. If, instead of being in the main so ruled, they were ruled *absolutely* by the sun, Leverrier's great work would have had no existence, as it would have had no utility. If the planets did not act upon each other by their attractive energies, any planet might be doubled or halved in mass, and all would go on unchanged. Nay, we might substitute for the eight chief planets as many peppercorns, and still the motions of these eight bodies would remain precisely the same. Calculated for one epoch, they would have been calculated for all time. No deviations would take place from which any inferences could be drawn as to the relative mass of the eight planets; but one continuous series of orbital circlings would go on, without change, for ever and ever.

But once recognise the fact that the planets disturb each other, and all this is changed. The more massive a planet is, the more potently will it disturb its neighbours. If we cannot tell exactly how much it does disturb its fellows, we can tell how large its mass is, compared with the earth's for example, which we may take as a convenient unit of reference. But it is clear that a planet's mass may be determined thus in many different ways. For instance, we may consider how much Venus disturbs the earth, and judge of Venus's

mass in that way; or instead, we may consider how much Venus disturbs Mercury, her next neighbour on the other side, and infer her mass in that way. We might also perhaps have an opportunity of seeing how Venus affected some unlucky comet which passed near to her, and thus obtain yet another determination of her mass. If these estimates did not agree, we should know there was something wrong either in our observations or in our calculations. We should be set on the track of some error. And it has been in this manner that science has almost invariably been set on the track of important truths. If we hunted down the error successfully, we should probably be led, not merely to correct that particular mistake, but also to discover some fact before unsuspected.

It is precisely in this way that Leverrier has dealt with the planetary motions. Taking first the seven chief planets known when his labours began, he set himself to inquire into their motions. He found before long that the tables hitherto in use did not accord rigorously with observation. Now, if every discrepancy had had a single cause, it would then have been a work of no small labour to determine each such cause. But the great difficulty which the astronomer has to deal with in considering the planetary perturbations resides in the fact that multitudinous causes are in operation, the effects of which are intermingled. Watch the troubled surface of a storm-swept ocean, and notice how every wave differs from its fellows in one respect or another, usually in many. Suppose now that the

task were assigned of analysing the causes of these varieties of forms. How difficult would the task be to distinguish one effect from another, when so many were manifestly in operation. A sudden gust of wind blowing against the sloping side of a great wave may aid to heap up or to depress the mass of water which at the moment forms the wave, and thenceforth through many oscillations the effect of that accident will remain. A wave under observation may have been affected by many gusts, acting in various ways. Again, a wave may be increased or diminished by combining with a cross-wave belonging to another series than the first, and such causes of change may have operated over and over again. Peculiarities of the sea-bottom act to modify the shape and size of waves, and a wave observed in one place may have been affected by such peculiarities in regions many miles away from the observer's station. It will be seen, then, that though the observer might find it an easy task to give a general explanation of the sea-waves before him, he would have a task of enormous difficulty—in fact, an altogether hopeless task—if he were asked to ascertain from the varieties of form presented by the waves, the peculiarities of all the modes of disturbance operative in giving to the waves their actual forms. Somewhat similar, though not altogether hopeless, as will soon appear, is the task of the astronomer called upon to assign to their several causes, *not* the observed perturbations—*that* would correspond only to explaining the general nature of the wave-motion—but the peculiari-

ties recognised in these perturbations, the various ways in which these differ from what may be described as their normal character.

It need scarcely be said that the motions of the earth herself have to be considered in this inquiry. I do not mean merely the motion of the earth on her orbit round the sun, but the disturbances which affect that motion. The earth herself is riding on the waves of perturbation. Her movement on these waves must be as carefully considered as her motion in her course. For not merely will that movement indicate directly the nature of those waves which particularly affect herself, but also, unless that movement is taken into account, the earth-borne observer will form an incorrect estimate of the waves by which the other vessels in sight are perturbed.

To this work, then, of determining exactly the characteristics of the earth's motion round the sun, Leverrier from the very outset of his inquiry devoted close attention. It need hardly be said that the method of dealing with the question was to study very carefully the sun's apparent motion from day to day, for this motion precisely corresponds with the real motion of the earth. It will give some idea of the extent of Leverrier's field of research, though but a faint idea of the nature of his work therein, to mention that, in dealing only with this one part of his subject, he reviewed and discussed nine thousand distinct observations of the sun, made since Bradley's time at Greenwich, Paris, and Königsberg. The first result which

attracted his attention was rather an unsatisfactory one. It is commonly supposed that the observations of the sun at those three observatories, and especially at Greenwich, have been so exceedingly precise as to leave nothing to be desired on that score. Bessel, of Königsberg, was led to remark, many years since, with some degree of surprise, that the theory of the sun (or, which is the same thing, the theory of the earth's motion) had not made the progress which might have been expected from so many and such accurate observations. Leverrier's opinion, which must be accepted as final, owing to the enormous number of observations he has examined and his unsurpassed skill as a mathematician, is very different. 'Our conclusion is,' he says, 'that the observations of the sun leave much to be desired, on account of systematic errors affecting them ; and there is no discordance between theory and observation which cannot be attributed to errors in observing.'

Yet Leverrier dealt so successfully with these observations, though thus imperfect, that he educed from them a noteworthy result. One class of disturbances affecting the earth's motion arises from the moon's disturbing influence. Its nature may be indicated by saying that in every lunar month the earth circuits around the common centre of gravity of her mass and the moon's. The diameter of this monthly orbit amounts to about six thousand miles, and as a result of this motion, she is about three thousand in advance of the centre of gravity just named when the moon is in her first quarter, and as far behind when the moon is in her third

quarter. Now it is that centre of gravity which alone
follows the true orbit around the sun which is attri-·
buted to the earth herself in the books. The earth no
more follows that orbit than the moon does. These
two bodies dance round and round each other (if we
may follow Sir John Herschel in using a rather homely
illustration), while the pair are swung round the mighty
mass of the sun. Of course this peculiarity of the
earth's real motion is reflected in the sun's apparent
motion. He seems at the time of the moon's first
quarter to be in advance, and at the time of her third
quarter to be behind, his mean place ; just as if *he*
were waltzing around in a monthly orbit six thousand
miles in diameter, while being also swung round in his
mighty annual path with its diameter of a hundred
and eighty millions of miles. But it is clear that, if
we can tell how large this apparent monthly orbit looks
as seen from the earth, we shall know how far off the
sun is. For the real size of this orbit is a matter
depending only on the earth and moon, and can be
inferred independently of the sun's distance. We
know, then, how large the path really is ; and if we
know how much the sun seems displaced in traversing
it, we have in fact learned how large a space of six
thousand miles looks when removed to the sun's dis-
tance. This is equivalent to determining the sun's
distance. Accordingly, Leverrier, having carefully
estimated the sun's apparent monthly displacements,
deduced thence an estimate of the distance of the sun,
and confidently informed astronomers, sixteen years ago,

that their accepted estimate of the sun's distance was too large by between three and four millions of miles.

This was not the first great result which rewarded Leverrier, though we have set it first because it followed from the inquiry which formed in a sense the basis of his whole system of researches. The first noteworthy result of his labours was that mentioned at the beginning of this paper, the discovery that the system of seven great planets was incomplete, another body, as yet unseen and unknown, travelling beyond the path of Uranus, and by its attraction disturbing the movements of that planet, for sixty years regarded as the remotest member of the sun's family.

And here, as in the case of the discovery of Uranus by Sir W. Herschel, good fortune as well as mathematical insight came into play. Herschel discovered Uranus by a lucky accident, when engaged in far other work than the search for new members of the solar family. Leverrier was not quite so lucky. He deliberately cast a line into space, hoping to capture the unknown disturber of Uranus. He satisfied himself by the most careful analysis of all available observations that Uranus really is disturbed by an unknown body (and, in passing, we may remark that in this respect Leverrier's work differed from that of Adams, who assumed this particular point). How then, it may be asked, was fortune concerned? I may illustrate the matter by the waves which we have already found convenient for such purposes. Suppose that an observer engaged in analysing a series of wave-disturbances

travelling (say) along a canal, observed some new class
of effects, as, for instance, that certain waves which had
long been of a particular size began to grow larger.
Suppose, that, struck by this, he instituted a careful
series of measurements of their size, and at last satis-
fied himself that they had increased. He might be
utterly at a loss to conjecture a cause. But if even
he conjectured a cause, as, for instance, some distur-
bance taking place at a part of the canal out of his
sight, he might still find it impossible to conjecture
how far off that part might be. If, however, while he
had satisfied himself by his wave-measurements that
the waves really had increased in size, he had also
satisfied himself that during his observations the
increase had reached its full extent, and had even
begun to give place to a slow decrease, tending to
restore the original size of the waves, he would mani-
festly have here an indication which might serve to
tell him of the very spot where the disturbance had
taken place. Eor example, the rate at which the waves
were travelling, combined with the time elapsed since
the peculiarity had been noticed, might indicate
exactly how many miles away was the scene of the
disturbance. Now something of this kind had hap-
pened in the case of Neptune. When astronomers
were thoroughly convinced that Uranus had been per-
turbed, or, in effect, when Leverrier had completed his
analysis (surpassing all others in completeness) of the
planet's observed motions, it had also become known
that the displacement had reached its maximum, and

was beginning slowly to decrease.   This showed astro-
nomers that the disturbing planet had made its nearest
approach to Uranus, and was now slowly drawing away.
Nor let the reader wonder that this was a process
requiring years to produce perceptible effects.   For
Uranus himself moves so slowly that he only completes
his circuit in 84 years, and Neptune (we now know)
requires more than 164 years; so that they come
sluggishly into conjunction and pass sluggishly out of
conjunction.[1]   Only when Adams and Leverrier began
to angle for the unknown planet had it become quite
certain that that body had been lately in conjunction
with Uranus.   If these astronomers had not known
when this happened within a few years either way, it
would have been utterly useless for them to have sought
for Neptune by mathematically analysing the distur-
bance affecting the movements of Uranus.   Their good
fortune consisted in this, that the conjunction had
opportunely occurred just when the motions of Uranus
were sufficiently observed to satisfy astronomers that
there was an external planet.[2]

[1] That is, they pass slowly into and away from the position in
which the sun, Uranus, and Neptune are nearly in a straight line.

[2] The general public, while underrating the mathematical difficul-
ties which Adams and Leverrier had to encounter, altogether overrated
the actual extent of the field over which Neptune had to be searched
for.   It was tolerably certain already that Uranus and Neptune had
been in conjunction between 1820 and 1825.   Between 1841 and 1846
then (i.e in 21 years) Uranus would have gone round a fourth of the
ecliptic as viewed from the sun; and the unknown planet probably about
half as far.   Neptune, then, was to be looked for near the ecliptic, and
about one-eighth of its circuit *behind* Uranus (both being supposed to
be viewed from the sun, which, in the case of planets so distant, is much

Setting, however, this piece of good fortune aside, which rendered their labours possible, the actual nature of the work of Adams and Leverrier was sufficiently arduous. And though their hypothetical Neptunes moved quite differently from each other, and departed still more widely from the path of the real Neptune, yet under the actual conditions, both astronomers were led, as we know, to point to a place very near to that occupied by the real Neptune at that particular time. It was as though, in the illustrative case just imagined, the observer had made some error in esti- mating the rate at which the wave-disturbance had travelled down the canal to his place, but yet guessed very nearly the true spot where it arose, because the time it had taken was but short : for instance, if the calculated rate were too great by half a mile per hour, but the time occupied were only twenty minutes, then he would only be in error by the sixth part of a mile. But if the time were, say, ten or twelve hours, then the error would be five or six miles. So Leverrier and Adams had their hypothetical Neptunes travelling too slowly by a quite appreciable amount : but yet, owing to the shortness of the time which had elapsed since Neptune and Uranus were in conjunction, the resulting error was very small ; and, as we know, the planet was found at the first cast of the telescopic line.

the same as viewing them from the earth). It was, in fact, tolerably certain before Adams and Leverrier began their calculations, that the unknown planet occupied a position somewhere on a known strip of the heavens not more than ten or twelve degrees long by about three degrees broad.

In passing to the next result of Leverrier's re-
searches, we have to turn from the outermost planets
of the solar system to Mercury, the one that, so far as
is as yet known, travels nearest to the sun.   The
motions of Mercury have been determined with a great
degree of accuracy, because Mercury often passes across
the face of the sun, and can at those times be observed
very exactly.   Now it was found that the observed
movements of this planet did not accord with those
calculated.   ' This result,' says Leverrier, quaintly
enough, ' naturally filled us with inquietude.   Had we
not allowed some error in the theory to escape us ?
New researches in which every circumstance was taken
into account by different methods, ended only in the
conclusion that the theory was correct, but that it did
not agree with the observations.   Long years passed,
and it was only in 1859 that we succeeded in unravel-
ling the cause of the peculiarities recognised.   We
found that they were all included under a simple law,
and that '—a certain slight change only was needed
to bring everything into order.   The nature of this
change was such as to indicate ' the existence of cos-
mical matter, as yet unknown, circulating like the
planets around the sun.   The consequence,' proceeds
Leverrier, ' is very clear.   There exists in the neigh-
bourhood of Mercury, doubtless between that planet
and the sun some matter as yet undiscovered.   Does it
consist of one or more small planets, or other more
minute asteroids, or even of cosmical dust ? [1]   The

[1] I follow in general a translation of Leverrier's paper in the

theory tells us nothing on this point. On numerous occasions trustworthy observers have declared that they have witnessed the passage of a small planet over the sun; but nothing has been established in this matter. We cannot, however, doubt the exactness of this conclusion.'

Such are Leverrier's latest utterances on this interesting question. He takes no notice, on the one hand, of the discoveries recently effected in meteoric astronomy, which demonstrate the existence of at least some matter in the sun's neighbourhood; nor, on the other, of the objections raised by Sir W. Thomson and others to the theory that large quantities of meteoric matter travel close by the sun. Nor does he speak of the singular statements made by the French doctor, Lescarbault, and once to some degree sanctioned by Leverrier himself, respecting the transit of a small black disc across the face of the sun on March 26, in the very year 1859, when Leverrier first laid his results respecting Mercury before the scientific world. We venture to quote Leverrier's account of his visit to Lescarbault's small observatory, as abridged from the *North British Review* for August 1860, in Chambers's useful treatise, 'Descriptive Astronomy.' It is well worthy of examination, whether it be regarded as evidence for the new planet—so confidently believed

' Monthly Notices of the Astronomical Society,' not having by me the original; but verbal changes have been made, the translation being, to say the truth, in very singular language. Leverrier, for instance, is made to say that '*a* matter exists in the sun's neighbourhood,' and to ask if it 'consists in cosmic dust.'

in once, that astronomers assigned a name to it, calling
it, appropriately enough, *Vulcan*—or as showing the
circumstantial way in which incorrect statements are
sometimes advanced :—

'On calling at the residence of the modest and
unobtrusive medical practitioner, Leverrier refused to
say who he was, but in the most abrupt manner, and
in the most authoritative tone, began, " It is then you,
sir, who pretend to have observed a new planet, and
who have committed the grave offence of keeping your
observation secret for nine months. I warn you that
I have come here with the intention of doing justice
to your pretensions, and of demonstrating either that
you have been dishonest or deceived. Tell me then
unequivocally what you have seen." The doctor then
explained what he had witnessed and entered into all
the particulars regarding his discovery. On speaking
of the rough method adopted to ascertain the period
of the first contact, the astronomer inquired what
chronometer he had been guided by, and was naturally
enough somewhat surprised when the physician pulled
out a huge old watch with only minute hands. It had
been his faithful companion in his professional journeys,
he said; but that would hardly be considered a satis-
factory qualification for performing so delicate an
experiment. The consequence was that Leverrier,
now beginning to conclude that the whole affair was
an imposition or a delusion, exclaimed with some
warmth, "What! with that old watch showing only
minutes, dare you talk of estimating seconds? My

suspicions are already too well founded." To this
Lescarbault replied that he had a pendulum by which
he counted seconds. This was produced, and found to
consist of an ivory ball attached to a silken thread,
which being hung on a nail in the wall is made to
oscillate and is shown by the watch to beat very nearly
seconds. Leverrier is now puzzled to know how the
number of seconds is ascertained, as there is nothing
to mark them ; but Lescarbault states that with him
there is no difficulty whatever in this, as he is accus-
tomed " to feel pulses and count their pulsations,"
and can with ease carry out the same principle with
the pendulum. The telescope is next inspected, and
pronounced satisfactory. The astronomer then asks
for the original memorandum, which after some
searching is found " covered with grease and lauda-
num." There is a mistake of four minutes in it when
compared with the doctor's letter, detecting which the
savant declares that the observation has been falsified.
An error in the watch (regulated by sidereal time) ac-
counts for this. Leverrier now wishes to know how
the doctor managed to regulate his watch by sidereal
time, and is shown the small telescope by which it is
accomplished. Other questions are asked and satis-
factorily answered. The doctor's rough drafts of
attempts to ascertain the distance of the planet from
the sun, from the period of four hours which is re-
quired to describe an entire diameter of that luminary,
are produced chalked on a board. Lescarbault's
method, he being short of paper, was to make his

calculations on a plank and make way for fresh ones by planing them off. Not being a mathematician, it may be remarked that he had not succeeded in ascertaining the distance of the planet from the sun. The end of it all was that Leverrier became perfectly satisfied that an intra-Mercurial planet had been really observed. He congratulated the medical practitioner upon his discovery, and left with the intention of making the facts thus obtained the subject of fresh calculations.'

This, however, was not the actual end of the matter ; for news came from an astronomer in Brazil, M. Liais, that at the very time during which Lescarbault said he watched the black spot crossing the face of the sun, he (Liais) was observing the sun, and nothing of the kind could be seen, though he was employing a telescope much more powerful than the one used by the French physician. It has also been pointed out that any planet nearer to the sun than Mercury ought to be a conspicuous object during total eclipse of the sun, whereas no such object has ever been noticed. On the whole it seems very doubtful how far the records of supposed transits can be trusted, and we seem almost compelled to adopt the opinion that the meteoric and cometic matter undoubtedly existing in the sun's neighbourhood in enormous quantities, produces the observed peculiarities in the motion of Mercury. In this case the united mass of all the meteoric matter within the orbit of Venus (not of Mercury, for Leverrier's result admits of explanation

by matter lying anywhere within about twice Mercury's distance from the sun) amounts, according to Leverrier's original estimate, to about a tenth part of the mass of Venus, or exceeds the mass of Mercury himself. This is not inconsistent with an exceeding tenuity of material. If the matter consists of small solid or liquid bodies, the sparseness of distribution would be very great. Suppose, for example, these bodies were of the same density as water; then together they would make a globe having about half the volume of the earth. Now, if they were scattered over a flat region shaped like a grindstone, extending all round the sun to Venus's distance, and having a thickness equal to the earth's diameter, this region would exceed the total volume of the scattered meteors no less than four hundred and thirty-five millions of times. So that, on the average, each meteor would have (wherein to disport itself free from contact or collision) a space exceeding its own volume to this degree. A meteor, for example, one cubic inch in volume, would have on the average a space equal in volume to a cube twenty-one yards in length and breadth and height. But the actual space occupied by meteors within the orbit of Venus is far greater, seeing that near the sun it has a thickness (so to speak of this disc-shaped region) of many millions of miles. Supposing the matter occupying this space to be a uniform gas, it would certainly be one hundred thousand million times rarer than water, or much more than a thousand million times rarer than air.

But it will presently appear that since Leverrier made that estimate of the mass of the disturbing matter, the estimate of our earth's mass, relatively to the sun's, has been increased by at least one-tenth part; and this would leave a much smaller quantity of matter to be provided by meteoric systems. There remains, however, sufficient evidence to show that the total mass of matter within the orbit of Mercury amounts, in all probability, to thousands of millions of tons.

I may remark here on an objection which has been urged by Sir E. Beckett (then Mr. Denison) in his fine work *Astronomy without Mathematics*, to the theory that vast quantities of meteoric matter in the sun's neighbourhood supply, as it were, the fuel or part of the fuel, by which the sun's fires are maintained. He showed that the quantity of matter necessary to produce this effect would be such that the sun would grow annually by a quantity equal to more than a twelve-millionth part (he gives exact numbers) of the sun's actual mass; and he proceeds to show that the effect of this would be to shorten the year by nearly one twenty-five millionth part of its length—that is, by about four seconds in three years. This would make our year shorter by about forty-seven minutes than the year in the time of Hipparchus, and we know quite certainly that there has not been a change even of half as many seconds. He proceeds then to touch on an objection to this reasoning, in the following words :—' If the meteors were all, before

their absorption within the earth's orbit, forming a sort of spherical extension of the sun, it is true that their joint attraction on the earth would be the same as after they had fallen into the sun. But I have seen no suggestion that this is so, and many meteor systems, especially the two largest that we know of, have orbits extending far beyond the earth's.'

This particular objection, or rather this reply to the original objection, had been advanced by me some years ago. Sir E. Beckett's answer does not seem to meet the argument. For all the meteor systems we can possibly become acquainted with (as such) are those encountered by the earth, and these form so minute a proportion of the total number (on any reasonable assumption of the probabilities) that it would be unsafe to reason from them. In fact, if we could, we might at once dismiss the meteoric theory of the sun's heat, because the two meteor-systems referred to by Sir E. Beckett do not pass within many millions of miles of the sun's surface. All the evidence we have, as I have elsewhere shown, indicates an increase in the density of meteoric distribution as we approach the sun, this increase becoming exceedingly rapid in the sun's immediate neighbourhood. Nor does it in the least matter that a certain proportion of the meteors thus crowded near the sun at any moment are in reality moving in paths carrying them far away from the sun. So long as the movements of the complete system are such that the gathering near the sun is permanent, though the mem-

bers composing it may be continually changing, the
consequences would be the same, or so nearly the
same as to make no appreciable difference in the
observed effects.

But there is in the very results on which the
meteoric theory h₁d been based—I mean Leverrier's
recognition of the existence of intra-Mercurial matter
—the strongest evidence that the sun's heat cannot
possibly be due entirely or chiefly to meteoric impact.
The quantity of downfalling matter necessary to main-
tain the sun's heat would be equal to about a fortieth
part of the earth's mass annually. Now Leverrier's
balance will not allow more than four times this
amount for the whole quantity of meteoric matter
within the orbit of Venus,—granting, that is, to the
region of greatest meteoric condensation the widest
permissible extension. So that there is only sufficient
matter to last for four years, if meteoric downfall were
the sole source of the sun's heat and the meteors were
to be continually used up for that purpose. Four
times four years have passed since Leverrier first pub-
lished his results, and neither has the sun grown cold,
nor the supply of meteoric matter perceptibly di-
minished.

Let us next turn to the results obtained by Le-
verrier when he put the planet Venus in the delicate
balance of analysis. Here we come again upon evidence
respecting the sun's distance, the theory of Venus lead-
ing, like the theory of the sun, to the conclusion that
the sun's distance had been over-estimated by three or

four millions of miles. But an interesting confirmation of the accuracy of Leverrier's theory of Venus is the point to which I would chiefly invite the reader's attention. Of course, on the occasion of the late transit, much depended on the accurate calculation of the time when Venus would cross the edge of the sun. The results satisfactorily proved the accuracy of the calculations. For instance, Mr. Hind found that, using the old tables of the sun and Venus, the calculated time of egress at Mokattam in Egypt differed by 13½ minutes from the observed time; whereas when Leverrier's new tables were used the calculated time was only five seconds in error. This is very satisfactory evidence of the value of Leverrier's labours.

We come, finally, to Mars, for the planets Jupiter and Saturn follow exactly the motions which theory ascribes to them.

One of the most interesting points, as it seems to me, in Leverrier's discussion of the motions of Mars is the fact that it indicates the wonderful power of mathematical analysis in dealing with matter, apart from all direct evidence as to the existence of such matter. Suppose no telescopic search had been made for the planet which astronomers of old time supposed to be travelling between the paths of Mars and Jupiter. Leverrier's analysis of the motions of Mars would in that case afford evidence decisive of the question whether a large but as yet undetected planet is really travelling in that region or not. It shows that there can be no such planet, simply because Mars shows no

traces of the disturbing influence of any considerable
planet. But Mars does show the influence of dis-
turbing matter, not giving him a strong pull in this
direction at one time and in that direction at another,
as a single planet would, but exerting a more equally
distributed action. This is the influence of the zone
of asteroids, and in this action we have a means of
weighing that zone.

But here, unfortunately, a difficulty arises. Leverrier
long since pointed out that the peculiar form of dis-
turbance thus affecting Mars might be explained either
by ascribing to the whole family of asteroids, when
taken together, a weight equal to one-eighth of the
earth's, or else by adding so much to the estimate of the
earth's weight. This last result corresponds almost
exactly with the effect of increasing the estimate of
the sun's distance to the degree indicated by Leverrier's
other researches. Some of our text-books, with their
usual happy freedom of manner, combine these two
results (stated by Leverrier in 1861), and assign to the
asteroids a total mass equal to one-eighth part of the
earth's, while *also* asserting that Leverrier's researches
on Mars, like those on Venus, proved that the earth's
mass must be increased by an eighth. But we cannot
assign the observed effects fully to both causes at once,
though we may assign part of the observed effects to
one cause and part to the other. Leverrier himself
does not, indeed, mention this. His words are as
follows:—' Only two hypotheses were possible, as we
explained on June 3, 1861; either the hitherto neg-

lected matter resided in the totality of the ring of small planets, or else it must be added to the earth itself. In the second case, and as a consequence, the distance of the sun must be diminished by about a twenty-fourth part of the ' (then) ' received value—that is, we are led to the result already obtained from the theories of the sun and Venus.' But then, if we ascribe the whole effect to the original erroneous estimate of the sun's distance, we are left in this predicament— that we can assign *no mass at all* to the whole family of asteroids.

Here, then, as in the case of Mercury, we see that we have to wait till the sun's distance is determined with much more exactness than heretofore, before we can ascertain the real results of Leverrier's planet-weighing. He has put these planets severally in the balance, and noted the result; but the balance itself has to be inquired into before we know what the result means. It can hardly be doubted that the transit observations made last December will come in very usefully at this point. We shall learn from them how much must be added to the old estimate of the earth's weight (or, which is exactly the same thing, how much must be taken from the old estimate of the sun's weight), and therefore we shall know how much is left, on the one hand, for intra-Mercurial matter, and, on the other, for the asteroidal family.

Now, it is somewhat strange that this being so,— Leverrier's own results pointing to the importance of

the direct measurement of the sun's distance by transit
observations, or in any other available manner,—he has
nevertheless spoken quite disdainfully of those direct
modes of measurement.   Because in weighing the
planets in his analytical balance, poised and adjusted
with marvellous skill, he has found clear evidence
that the old measurements of the sun's distance were
erroneous, he deprecates new measurements. 'Here
I have,' he says in effect, 'a way of testing such
measurements so delicate that in itself it is preferable
to them all.   The balance I have used is one which
will improve with advancing years, and as, in 1861,
it had detected the error in measurements of the sun's
distance effected in 1769, so, long before the transits of
the twenty-first century, it will have given results
altogether more accurate than those you are attaining
at so much expense by observing the transits of the
present century.'   This is all very well; but Leverrier's
own results leave something to be explained which
these despised transit observations are competent to
explain at least a good deal more accurately than he
has himself explained them.   His method, carefully
kept in bottle for another half-century, may, and pro-
bably will, give us a much clearer wine (to use Bacon's
simile), but in the meantime we must be content with
the vintage of 1874 and 1882.

But this in no sense affects the value of Leverrier's
own labours.   Beyond question he has deduced from
the observed motions of the planets all that at present
can be deduced as to the masses of the different known

and unknown parts of that complex system,—containing
bodies of all orders of size, density, and structure,—
which occupies the domain of space ruled over by the
sun. We spoke of his work, begun more than a third
of a century ago, as the noblest work in pure astro-
nomy which this age has seen. This certainly seems
no exaggerated estimate of its value. A portion only
of the work—that which led to the discovery of Nep-
tune—has been called the greatest achievement of
mathematical astronomy since Newton's discovery of
the law of gravitation. As regards this portion of his
labours, his credit is shared by another astronomer not
less skilful than Leverrier, though circumstances have
prevented him from pursuing his course along the
difficult path for which his powers fit him. Other
astronomers, again, have shared with Leverrier the
labour of analysing the movements of particular
planets, or rather have gone over the same ground with
somewhat similar results. But as Sir John Herschel
alone of all astronomers ever surveyed with high tele-
scopic powers the whole of the star-lit sphere surround-
ing our earthly home, so Leverrier alone has submitted
to the searching scrutiny of the higher mathematical
analysis the whole of that complicated system to which
the earth belongs. It adds not a little to the credit
due to him for these achievements that during the
greater part of his labours he held a high official post,
the duties of which (had he been content to follow an
example but too common) might well have exonerated

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him from the continuance of independent labours so
arduous and exacting.

COMETS' TAILS.

WHEN we consider the surprising nature of the pheno-
mena presented by the tails of comets, we can scarcely
wonder that the most startling theories have been sug-
gested in explanation.   Their whole behaviour is ano-
malous.   The head of a comet, or rather the bright al-
most point-like nucleus, obeys the law of gravity ; and
wonderful though the nature of the comet's orbit some-
times is, extending into depths so remote that the mind
shrinks from pursuing the comet on its journey through
them, there is not a mile of the comet's voyage which
does not exemplify in the exactest manner the laws re-
cognised by Newton.   But it is quite otherwise with
the tail, if we regard the tail as a material object
carried along with the comet.   The end of the tail, for
example, shifts through space with a velocity such as
the sun could not possibly generate by his attractive
influence, mighty though that influence is, nor control
if otherwise generated.   Cometic tails are flung forth
from the head, or at least appear to be flung forth, with
a rapidity far exceeding even the tremendous velocity
with which a comet, passing near the sun, sweeps
round that orb at the time of nearest approach.   Then

the varieties of appearance presented by comets' tails, the singular changes of shape in one and the same tail, the existence of more tails than one, and a number of other strange circumstances, seem to defy explanation, and so to invite the wildest speculation.

I propose to consider here some of the more promising attempts which men of science have made to solve the mystery of comets' tails, and to touch also on some ideas which, though advanced by persons more or less distinguished in various departments of science, appear on examination to be untenable.

It is manifest that if we would form a just theory of cometic appendages, we must pay special attention to their more remarkable characteristics, because in this way we shall be able to get rid of innumerable theories, accounting fairly enough for ordinary appearances, but irreconcilable with those of a less usual nature. But we must also closely consider those features which, though usual with the objects we are considering, are remarkable in the sense that they distinguish these objects from others.

Take, then, first of all the fact that ordinarily the tail of a comet extends from the head in a direction almost exactly opposite to that in which the sun lies, or, in fact, has very nearly the position which the shadow of the head would have if a comet were of such a nature as to cast a shadow outwards into space. Being luminous, instead of dark, a comet's tail has been described on account of this peculiarity as a *negative shadow.*

If comets' tails were always of moderate dimensions

we might readily enough conceive that their position was not inconsistent with the supposition that they are material appendages, unchanging in constitution though changing in position. Some form of repulsion exerted by the sun on such appendages might (after the manner seen in certain electrical experiments) keep them always streaming out on the side farthest from him.

But the enormous dimensions of cometic tails, as well as their wonderfully rapid formation, extension, and changes of figure, will not permit us to adopt such a theory for an instant. The consideration of a single instance will show conclusively that a comet's tail must be otherwise explained. I take, as one of the most remarkable cases of the kind, the comet of 1680. This comet was invisible for four days during the time of its nearest approach to the sun. All this time it was circling rapidly round; in other words, it was swiftly changing its direction of motion, and its position with respect to the sun. When it first became visible after this rapid movement it was passing away from the sun in a course nearly opposite in direction from that by which it had arrived. And now, *carried in front* of the retreating comet, there was a tail more than ninety millions of miles in length. So far as appearances were concerned this tail was the same with which the comet had approached the sun, only it seemed to have been carried almost completely round until now it had nearly the same direction as it had when the comet was approaching the sun. But it could not really have been brandished round in this way, simply because the course

which, on this supposition, the end of the tail would
have had to follow, would have required a velocity of
motion incomparably exceeding any which the sun's at-
traction could account for. Moreover, a material tail
of such dimensions, even though composed of a sub-
stance millions of times stronger than steel, would have
been rent into fragments by the tremendous forces
called into play in a whirling motion of the kind.
Knowing, as we do, that the tail of a comet has hardly
any substance at all, insomuch that, despite its enor-
mous volume, it produces no disturbing effect by its
attraction even on the smallest members of the solar
system, we see how utterly incapable so tenuous a tail
would be to bear the strain resulting from the ima-
gined motion. In reality, however, the supposition is
one which does not need serious refutation.

But if we suppose, as we seem forced to do, that
this tremendous tail, seen after the comet had swept
around the sun, was a *new* formation, swept out into its
observed position by some mighty repulsive force ex-
erted by the sun, we must adopt the most startling con-
ceptions of the activity of that force. Under gravity
the comet's nucleus, although when approaching the
sun it arrived at the earth's distance from that orb
with a velocity of about twenty-five miles per second,
required four more weeks to complete its journey to the
point of nearest approach; whereas here was a tail equal
in length to the earth's distance from the sun flung
forth in less than four days. Nay, from the observed
direction of the tail and its subsequent changes of

position, it became manifest that a few hours had
sufficed to carry the material of its extremity (on the
repulsion theory) from the nucleus to that distant
position.

It was in order to get over this difficulty that
Professor Tait devised the ' sea-bird theory ' of comets'
tails.   If we watch a flight of birds travelling nearly in
a plane, at a great distance, we notice that when the
eye is nearly in the level of the plane, the flight appears
like a well-defined streak above the horizon ; but if the
plane is so situated that the eye is above or below its
level, the flight of birds can hardly be discerned at all.
Another phenomenon more frequently observed may
also serve to illustrate Professor Tait's theory.   We may
often see a long, straight, and well-defined cloud to-
wards the horizon which is really the edge-view of a
thin, flat cloud of fleecy structure, as we may see by
comparing it with others seen above it higher and
higher, even to the part of the sky overhead where we
look directly through the thinnest part of such clouds.
Comparing the dense-looking though narrow cloud on
the horizon with the filmy appearance of the cloud
overhead, one would not suppose the two could be alike
in structure, were it not for the gradual change of ap-
pearance of the intermediate clouds as we direct the
sight from the horizon upwards.   The clouds illustrate
what Professor Tait says as to the difference of visibility
between an edge-view and a thwart-view of a plane of
discrete bodies like his sea-birds.   But we must return
to the birds themselves to understand his actual appli-

cation of the phenomenon. It will sometimes happen that a flight of birds viewed athwart will, by a slight and rapidly effected change of position, present the edge-view, and thus change in a few moments from an indistinct cloudlike aspect to the appearance of a sharply defined and heavy streak upon the sky; or, a flight of birds absolutely invisible in the former position, may thus become in a few moments clearly visible, and extending to a great apparent length.

Professor Tait considers that a tail like that of Newton's comet, instead of being thrown out in a few hours as had been supposed on the repulsion theory, may simply have become visible in the manner of a flight of sea-birds travelling as just described.

When we remember that Professor Thomson, when President of the British Association at Edinburgh, spoke enthusiastically of the simplicity and beauty of Tait's sea-bird theory, Tait being on that occasion President of the Section of Physical Science and Mathematics; and when we further remember that both Thomson and Tait are deservedly eminent for their skill in mathematics (the very soul, as it were, of astronomy), we are unable to receive otherwise than respectfully a theory so strongly supported by authority. And yet this theory is so utterly unsupported by evidence from the observed appearance or behaviour of comets that we are compelled to regard its invention by Tait, and its acceptance by Thomson, as having little relation to the actual subject of cometic astronomy. All that can be admitted is indeed simply all that Professor Tait has

attempted to show.   Given a shoal of meteors ninety
millions of miles long and viewed slightly athwart, the
shoal, invisible as so situated, might in a few hours be-
come visible along its whole length, and its rapid ap-
parition would correspond with the apparently rapid
formation of the tail of Newton's comet after the comet
had been circling close around the sun.   But how the
shoal of meteors came at this time to be in front of
the comet, whereas on the sea-bird theory the comet
had approached the sun with a shoal of meteors ex-
tending millions of miles behind it ; why the shoal was
visible all the time that the comet was visible, both in
approaching and in receding ; why this edge-view of a
shoal was millions of miles thick and utterly unlike
such a shoal on any conceivable supposition as to its
structure ;—these, and a hundred other such questions
suggested by the different, the changing, and the com-
plex appearances presented by various comets, find no
answer in the sea-bird theory.   Until some attempt has
been made to reconcile this theory with these pecu-
liarities, the theory can hardly be regarded as seriously
advanced.   In the meantime I venture to say that no
shoal of meteors can be made to account for the appear-
ances presented by comets' tails under any amount of
mathematical manipulation.

But there is something so startling in the concep-
tion of a repulsive energy competent to account for the
formation of comets' tails, that one naturally seeks for
any explanation which may account for the phenomena
without forcing upon us the idea of so amazing a force.

Especially is this the case when we consider that, on
the theory of repulsion, the old tails, enormous though
their dimensions are, must be regarded as continually
dissipated into space ; so that we have to suppose a
series of tails, each many millions of miles in length,
and of vast breadth and thickness, all formed from out
of the substance of the comet, and swept for ever away
from it. It is easy indeed to speak of the retreating
comet gathering its substance together when once be-
yond the domain of the sun's repulsive power ; but the
velocity with which that substance is swept away is such
as not even the sun himself could overcome by his at-
tractive energy : much less could the feebly attracting
head of a comet draw back the stragglers which the
sun's replulsion had (on this theory) hurried away into
surrounding space.

We can understand, then, that students of astro-
nomy, observing the fact that the comet's tail is di-
rected from the sun, much as a shadow would be, should
be led again and again to discuss the inviting theory
that the tail is a species of negative shadow. This
theory has commonly been presented somewhat on this
wise :—The head of a comet is regarded as acting the
part of a lens, in such sort that the sun's light is poured
into the region behind the comet more richly than
elsewhere. Now, if this region were absolutely vacant,
the light thus streaming behind the comet would pro-
duce no visible effect : it would illuminate any ma-
terial substance which happened to be there ; but if
there was nothing there, then the blackness of inter-

stellar space would prevail in this region as elsewhere. Accordingly the lens theory of comets requires that some matter should be supposed to exist behind the comet (considering the sun as in front); and as the comet takes up in succession many different positions with respect to the sun, we require to have matter all round the comet's head to a distance equal to the observed length of the tail. Either we must regard this matter as belonging to the comet, or as belonging to the solar system. If we take the former view, we should have to suppose that many comets have had the most astonishing dimensions. Newton's, for example, must have been nearly 200 millions of miles in diameter. This is not merely incredible, but impossible, because there would be nothing to retain this enormous sphere of tenuous matter around the central nucleus except the attraction of the nucleus, which we know to be exceedingly feeble from the fact that the smallest planets and satellites are in no way disturbed even by the near approach of the largest comets. Taking Newton's comet, the nucleus of which came within less than a quarter of a million miles of the sun, there was the sun himself at this time in the very heart of the enormous sphere of matter over which the utterly insignificant mass of the nucleus is supposed on the lens theory to have borne sway. The comet could never have carried away from the sun's neighbourhood its attendant sphere of matter.

Much more conceivable is the theory that the matter illuminated by the light streaming behind the

comet belongs to the sun's domain, and is always present, ready to be illuminated so soon as a comet-lens comes into a suitable position. But in reality the known laws of optics present overwhelming objections against this inviting theory. Supposing for a moment that a comet were able to condense the light behind it in the particular manner which the theory requires, the light thus streaming backwards would form a perfectly straight tail. For although a series of bodies continually setting out from the comet at a moderate velocity in a direction away from the sun would form a curved tail, simply because the comet is all the time moving onwards upon a curved path, yet light travels with such enormous velocity that the longest cometic tail ever seen would be traversed in a few minutes, and in so short a time the comet would not have advanced appreciably on its curved path.[1] There would not be the slighest visible curvature, therefore, in the tail. If this reasoning seem unsatisfactory to the reader, without diagrams and elaborate explanations, then let him consider the simple fact that comets have had more tails than one, and tails quite differently shaped and placed (a strongly curved tail beside one or two perfectly straight tails): this circumstance is manifestly sufficient to overthrow the lens theory of comets' tails.

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[1] It would have advanced many thousands of miles no doubt, but the direction of its motion would not change appreciably. Though the earth travels 60,000 miles an hour, it takes a whole day to change the direction of her motion a single degree.

Professor Tyndall was led by his researches upon light to a theory somewhat similar to the lens theory, but altogether better worthy of careful consideration.

He had noticed during his experiments on the chemical action of light· that almost infinitesimal amounts of matter when diffused in the form of a cloud can 'discharge from it by reflection' an astonishing body of light. Let us first understand the exceeding minuteness of the quantities of matter employed in Tyndall's experiments. Having first assured himself of the perfect purity of the tube (3 feet long by 3 inches wide), by so cleansing it that when filled with air, or the vapour of aqueous hydrochloric acid, the most intense light falling on it would not produce the least cloudiness, he proceeded as follows :—' I took,' he says, 'a small bit of bibulous paper, rolled it up into a pellet not the fourth part of the size of a small pea, and moistened it with a liquid possessing a higher boiling-point than that of water. I held the pellet in my fingers until it had become almost dry, then introduced it into' a small pipe serving for the introduction of gas into the main tube, 'and allowed dry air to pass over it into this tube. The air charged with the modicum of vapour thus taken up was subjected to the action of light. A blue actinic cloud began to form immediately, and in five minutes the blue colour had extended quite through the experimental tube. For some minutes this cloud continued blue . . . . but at the end of fifteen minutes a dense white cloud filled the tube. Considering the amount of vapour carried

in by the air, the appearance of a cloud so massive
and luminous seemed like the creation of a world out of
nothing.'

But this was far from being all   Minute as was
the quantity of light-generating vapour now present in
the tube, it was largely reduced before the next experi-
ment was made.   ' The pellet of bibulous paper was re-
moved and the experimental tube was cleared out by
sweeping a current of dry air through it.   *This current
passed also through the connecting piece in which the
pellet of bibulous paper had rested.*   The air was at
length cut off and the experimental tube exhausted.'
Then the tube was again filled by the vapour of hydro-
chloric acid, which had passed through the connecting
piece.   Now let it be noted how exceedingly, almost
infinitesimally, minute was the quantity of light-gene-
rating matter remaining in the tube.   For, first, the
pellet of bibulous paper had absorbed but a minute
quantity of liquid ; secondly, nearly the whole of what
had been absorbed had been allowed to evaporate before
the pellet was put into the connecting piece; and,
lastly, ' the pellet had been ejected, and the tube in
which it rested had been for some minutes the conduit
of a strong current of pure air.'   The matter now to
be experimented upon was ' part of such a residue as
could linger in the connecting piece after this process,'
and had been now carried into the 3-feet tube by the
hydrochloric acid.   Yet the effects were remarkable
when the electric lamp was allowed to pour its light
upon the tube.   ' One minute after the ignition of the

lamp,' says Tyndall, 'a faint cloud showed itself; in two minutes it had filled all the anterior portion of the tube and stretched a considerable way down it; it developed itself afterwards into a very beautiful cloud-figure; and at the end of fifteen minutes the body of light discharged by the cloud, considering the amount of matter involved in its production, was simply astounding. But, though thus luminous, the cloud was far too fine to dim in any appreciable degree objects placed behind it. The flame of a candle seemed no more affected by it than it would be by a vacuum. Placing a table of print so that it might be illuminated by the cloud itself, it could be read *through* the cloud without any sensible enfeeblement. Nothing could more perfectly illustrate that "spiritual texture" which Sir John Herschel ascribes to a comet than these actinic clouds. Indeed the experiments prove that matter of almost infinite tenuity is competent to shed forth light far more intense than that of the tail of comets. The weight of the matter which sent this body of light to the eye would probably have to be multiplied by millions to bring it up to the weight of the air in which it hung.'

It may fairly be said that Tyndall's luminous cloud is the only terrestrial object yet known to physicists which fairly illustrates the phenomena presented by comets' tails as respects their extreme tenuity and the quantity of light they nevertheless discharge. This is a somewhat important point in any theory of these mysterious objects, and it does not appear to me that

astronomers (who have not been altogether successful in determining the nature of comets from their telescopic researches) ought to look askance at physical facts which strikingly illustrate cometic phenomena, merely because those facts were not discovered with a telescope.

Let us see, however, how Tyndall associates his actinic clouds with comets and their appendages.

After briefly describing the difficulties which surround cometic phenomena, he proceeds to present 'a speculation which seems to do away with all these difficulties, and which, whether it presents a physical verity or not, ties together the phenomena exhibited by comets' (he should rather have said, *many* of the phenomena) 'in a remarkably satisfactory way :—The theory is, that a comet is composed of vapour decomposable by the solar light, the visible head and tail being an actinic cloud resulting from such decomposition; the texture of actinic clouds is demonstrably that of a comet. The tail is not projected matter, but matter precipitated on the solar beams traversing the cometary atmosphere. *It can be proved by experiment that this precipitation may occur either with comparative slowness along the beam, or that it may be practically momentary throughout the entire length of the beam. . . . As the comet wheels round . . .* the tail is not composed throughout of the same matter, but of new matter precipitated on the solar beams, which cross the cometary atmosphere in new directions. . . . The tail is always turned from the

sun for the following reason:—Two antagonistic powers
are brought to bear upon the cometary vapour—the
one' a chemical power tending to form the invisible
cloud, the other a heating power tending to dissipate
it into invisible vapour.   'As a matter of fact, the sun
emits the two agents here involved.   There is nothing
hypothetical in the assumption of their existence.'
That visible cloud should be formed behind the head,
or in the space occupied by the head's shadow, it is
only necessary to assume that the sun's heating rays
are absorbed more copiously by the head than the
chemical rays.   This augments the relative superiority
of the chemical rays behind the head, and enables them
to form the visible cloud which constitutes the tail.
The old tail, so soon as the head by its onward motion
ceases to screen it, is dissipated by the sun's heat.
The dissipation, like the formation, not being instan-
taneous, the curvature of the tail and the direction
of the curvature are accounted for.   Other peculiari-
ties are shown to be explicable by the theory; and, in
particular, Tyndall remarks that 'the cometary en-
velopes and various other appearances may be accu-
rately reproduced · through the agency of cyclonic
movements introduced by heat among' the chemical
clouds with which the theory has to deal.[1]

[1] The following remarks by Tyndall suggest strange possibilities:—
'There may be comets,' he says, ' whose vapour is undecomposable by
the sun, or which, if decomposed, is not precipitated. This view opens
out the possibility of invisible comets wandering through space, perhaps
sweeping over the earth and affecting its sanitary condition without our
being otherwise conscious of their passage. As regards tenuity, I enter-

There are many strong points in this theory, and it shows to great advantage, in particular, by comparison with one which, as we have seen, found special favour with mathematicians—the sea-bird theory. It not only explains the facts which had suggested it, but is shown by its author to accord with many characteristics of comets, some among them being such as had been long considered most perplexing.

A comet, however, which astronomers were able to study more thoroughly than any other ever known seems to me to have afforded decisive evidence in favour of the repulsion theory of comets' tails, and against the ingenious theory just described. I refer to Donati's comet, or the comet of 1858–59.

This remarkable object, like most large comets, presented the appearance of concentric envelopes around the head. These were apparently raised by the sun's heat, and each after being formed rose gradually farther and farther from the nucleus, being succeeded after it had reached a certain distance by another envelope, this by another, and so on ; so that at the time of greatest development three well-marked envelopes were simultaneously visible, besides the gradually fading remnants of two or three others. The great curved tail which formed so remarkable a feature of

tain a strong persuasion that out of a few ounces (the possible weight assigned by Sir John Herschel to certain comets) of iodide of allyl vapour, an actinic cloud of the magnitude and luminousness of Donati's comet might be manufactured.

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that comet presented the usual appearance of being
formed by the sweeping away of the outer parts of the
envelope by a solar repulsive force; and its well-marked
curvature showed that if such a repulsive force had
really acted, the rate at which it swept the matter of
the tail outwards, though very rapid, was by no means
so rapid as the motion of light.   The tail visible at
any given time (during the chief splendour of the
comet) was the work of several days, not of a few
minutes, whether the repulsion theory or Tyndall's
were the true explanation.   But now, as if to illustrate
what Tyndall says of the various rates at which the
chemical cloud may be formed and dissipated (see the
last two italicised passages in our account of his theory),
a straight tail became visible beside the curved one.
It was not visible in England, but was well seen in
America.   This, of course, was in agreement with the
repulsion theory also, since it only required that the
comet's head should be regarded as consisting of two
kinds of matter, one kind undergoing repulsion with
exceeding swiftness, so as to form the straight tail, the
other repelled with a more moderate velocity, and so
forming the curved tail.

So far, then, there was no special reason for pre-
ferring either theory.   But now a circumstance was
noted which, so far as I can see, the repulsion theory
is alone competent to explain.   I must note that
the reasoning which follows, though it presented itself
independently to the present writer, was long before
adduced by Professor Norton, of Yale College, in

America, the well-known author of the auroral theory
of the solar corona. The great mass of the matter
undergoing repulsion was carried into the large, bright,
curved tail. We can conceive that in thus moving off,
this matter, being so much greater in quantity, would
be apt to carry off along with it, and, as it were,
entangled in its substance, portions of the matter
which should have gone into the small tail—the
matter, namely, on which the sun's repulsive action
was able to act more swiftly, sweeping it out into
straight lines. The matter thus carried away into the
wrong tail, as it were, would be always ready to escape
from the entanglement so soon as the matter which had
carried it off began, through wide-spreading, to leave
it free. And then at once the sun's repulsive action
would act upon this matter precisely as on the matter
of the same kind forming the straight tail ; it would
repel this matter, which had escaped from the entang-
ling matter of the curved tail, and sweep it away in a
straight line, so that it would form, as it were, a sort
of subsidiary tail, not extending from the head, but
from a particular part of the curved tail. This
happening from time to time, the curved tail would
manifestly have a number of straight tails, or streamers,
all extending on the same side of it as the straight
tail which streamed from the head. Now this was
precisely the appearance presented by the curved tail
of Donati's comet—a sort of combing out, or striation,
the direction of the different streamers corresponding

exactly with that which would result from the mode of formation just described.

It is difficult to see how Tyndall's theory can be reconciled with this peculiarity of appearance. For, if we regard the straight tail as formed by the sun's chemical rays, a portion of his heat rays being absorbed by the action of part of the head, it would be necessary to suppose that the other straight tails—the streamers, that is, from the great curved tail—were similarly formed. If this were so, then at various points along the length of the curved tail there must have been matter of the same nature as that matter in the head to which the chief straight tail is attributed. But this looks very like admitting that the great tail consisted partially of matter repelled from the head; and if we admit repulsion at all, we may as well admit it as entirely operative. We are not indeed bound to do so; in fact, in my opinion, one of the most serious mistakes which modern theorisers in all departments of science are apt to make, is the endeavour to explain phenomena as due to one or other of two or more causes, when in reality both causes or several may be in operation. Still it is manifest that, in the present case, the only positive evidence is in favour of the repulsion theory, since, starting even from Tyndall's theory, we find evidence of the repulsion of matter from the head into the great curved tail.

I have said nothing here of the meteoric theory of comets,[1] because, so far as is known, it is the head

[1] This theory has been very fully dealt with in the ' Borderland of Science.'

only of comets to which that theory applies. It is known that meteors follow in the track of the head, that is, in the same orbit; but the tail does not at any time agree in position with the orbit, and we have no sufficient reason from observation to suppose that the tail consists of meteoric matter, although of course it is quite possible that the repulsion by which the tail seems to be formed may carry into the tail matter of the same sort as that out of which the meteoric attendants are formed.

The observations made with the spectroscope and with the polariscope upon the comet which so lately adorned our skies have not thrown any noteworthy light on the subject. It has been shown that part of the light of the tail gives the same spectrum as the small comets heretofore observed—a spectrum somewhat hastily associated with that of carbon—and that part of the light is probably reflected sun-light. But the observations have been imperfect and unsatisfactory.

We may still say, as Sir John Herschel long since said,—' There is, beyond question, some profound secret and mystery of nature concerned in the phenomena of comets' tails. Perhaps it is not too much to hope that future observation, borrowing every aid from rational speculation, grounded on the progress of physical science generally (especially those branches of it which relate to the ethereal or imponderable elements), may ere long enable us to penetrate this mystery, and to declare whether it is really *matter*, in the ordinary acceptation of the term, which is projected

from their heads with such extravagant velocity, and
if not impelled, at least directed in its course by a
reference to the sun as its point of avoidance.'

THREE ORDERS OF COMETS.

SOME of the facts of science are stranger than any
fictions which even the liveliest imagination could
devise. So strange are they that even the student of
science who has been engaged in the work of master-
ing them is scarcely willing to admit their full
significance, or to accept all the inferences which
are directly or indirectly deducible from them. This,
true in all departments of science, is especially note-
worthy in astronomy; and perhaps there is no branch
of astronomy in which it is more strikingly seen than
in that which relates to comets. During the last
quarter of a century discoveries of the most surprising
nature have been made respecting these mysterious
bodies; relations have been revealed which bring them
into association with other objects once regarded as of
a totally different nature, and the path seems open
towards results yet more amazing, by which, more than
by any others which even astronomy has disclosed, we
seem brought into the presence of infinite space and
infinite time. The earth on which we live—nay, our
solar system itself—seems reduced to utter insignifi-

cance compared with the tremendous dimensions of
comet-traversed space; while all the eras of history,
and even those which measure our earth's existence,
seem as mere seconds compared with the awful time-
intervals to which we are introduced by the study of
cometic phenomena.

One of the most interesting points suggested by
the recent cometic discoveries is the question, how
comets are to be classified. That they are not all of
the same order is manifest, whether we consider their
size, or the shape and extent of their orbits. But pre-
cisely as in zoological classification mere size or de-
velopment is considered a much less important point
than some really characteristic difference of structure,
or even than a difference of distribution, so in classi-
fying comets it would be unsatisfactory in the extreme
could we have no more characteristic difference to
deal with than that of dimensions. Supposing, for
instance, that we could separate comets into those
with or without a nucleus, or those with or without
a tail; such a classification, if it was found to corre-
spond with a real difference of nature, would be much
more satisfactory than the arrangement of comets into
various orders differing only in size. One of the most
interesting questions, then, in the cometic astronomy
of a few years ago was this—Are the peculiarities just
referred to—the absence or presence of a nucleus, or of
a tail—really characteristic, or do they correspond to
mere differences of development? I say that this ques-
tion belonged to the cometic astronomy of a few years

ago, though even then there were reasons for regarding
the various forms of structure observed in comets as
depending only on development. Of course comets
which during the whole time of their visibility, showed
neither tail nor well-defined nucleus, could afford no
means of answering the question. But a comet like
Donati's—the glorious plumed comet of 1858—which
appeared as a mere globular haze of light, and gradu-
ally during its approach to the sun assumed one form
after another of cometic adornment—the nucleus, the
fan-shaped expansion, the long curved tail, striations
within the tail and envelopes outside the fan, while
finally even subsidiary tails made their appearance—
teaches us unmistakably that these features depend
merely on development. We might as reasonably
place the chicken in another class than the full-grown
fowl because it has neither comb nor coloured tail-
feathers, as set a small comet in another order than
that to which Donati's belongs because the small one
shows neither tail nor coma. The gradual loss of
these appendages by Donati's comet, during its retreat
into outer space, of course strengthens this view. But
perhaps the most remarkable proof ever afforded of
the variety of appearance which the same comet may
present, was that given by Halley's comet at its return in
1835–36 ; for on that occasion, after showing a fine
coma and tail during its approach towards the sun, it
was seen in the southern hemisphere by Herschel and
Maclear, not only without tail, but even without coma,
appearing in fact precisely like a star of the second

magnitude. After this—that is to say, during its
retreat—it gradually resumed its coma, and even
seemed to be throwing out a new tail, but no complete
tail was formed while the comet remained visible.

Indeed the difference between the appearance pre-
sented by the same comet before and after its nearest
approach to the sun is not only remarkable in itself, but
subject to remarkable variations. 'What is very
remarkable,' says Sir John Herschel on the first point,
' the shape and size are usually totally different after the
comet's reappearance (on the other side of the sun)
from what they were before its disappearance. Some,'
he remarks on the second point, 'like those which
appeared in 1858 and 1861, without altogether dis-
appearing as if swallowed up by the sun, after attain-
ing a certain maximum or climax of splendour and
size, die away, and at the same time move southward,
and are seen in the southern hemisphere, the faded
remnants of a brighter and more glorious existence of
which we here witnessed the grandest display; and on
the other hand we here receive as it were many comets
from the southern sky, whose greatest display the in-
habitants of the southern parts of the earth only have
witnessed. It also very often happens that a comet,
which before its appearance in the sun's rays was but
a feeble and insignificant object, reappears magnified
and glorified, throwing out an immense tail, and ex-
hibiting every symptom of violent excitement, as if
set on fire by a near approach to the source of light
and heat. Such was the case with the great comet of

1680, and that of 1843, both of which, as I shall
presently take occasion to explain, really did approach
extremely near to the body of the sun, and must have
undergone a very violent heat. Other comets, fur-
nished with beautiful and conspicuous tails before their
immersion in the sun's rays, at their reappearance are
seen stripped of that appendage, and altogether so very
different, that but for a knowledge of their courses it
would be quite impossible to identify them as the same
bodies. Some, on the other hand, which have escaped
notice altogether in their approach to the sun, burst
upon us at once in the plenitude of their splendour,
quite unexpectedly, as did that of the year 1861.'

It was clear, then, long since, that comets cannot
be classified either according to their size or their de-
velopment. But this has been even more conclusively
shown by the spectroscopic analysis of large and small
comets. For certain bright bands seen in the spectra
of the small comets which had been examined before
the present year, are found also to characterize the
spectrum of the comet which adorned our northern
skies in June and July 1874, and to be shown not only
by the coma, but also by the tail. I do not here enter
into any special consideration of the results of spectro-
scopic analysis as applied to this comet, because to say
truth our spectroscopists have not met with any
noteworthy success; nor have the spectroscopists of
the southern hemisphere sent in statements from
which we can determine whether any special accession
has been made to our knowledge. It may, however,

be assumed from what has been observed here, that the characteristic spectrum of comets, large and small, is that three-band spectrum which was first recognised during the spectroscopic investigation of Tempel's small comet in the year 1866.

Comets, then, must be classified in some other way. It is not difficult to select the proper method of classification—a method not only satisfactory as respects the distinctions on which it depends, but exceedingly suggestive (as, in fact, every just mode of classification may be expected to be).

I would divide comets into three classes, according to the nature of their paths.

First, there are the comets which have paths so moderate in extent that their periods of revolution belong to the same order as the periods in which the planets revolve around the sun. This class includes all the comets which have been described as Jupiter's comet-family, and all those similarly related to Saturn, to Uranus, and to Neptune. Other comets of somewhat greater period than Neptune's comet-family may perhaps be regarded as associated with as yet undiscovered planets revolving outside the path of Neptune, and therefore as belonging to the same family. I would not, however, attempt to define very narrowly the boundary of the various classes into which comets may be divided, and in what follows I shall limit my remarks to comets which are clearly members of one or other class, leaving out of consideration those respecting which (for want, perhaps, of more complete

information than we at present possess) we may feel
doubtful.

Secondly, there are comets of long periods, but
which yet show unmistakably by their motions,
that they are in reality members of the solar system—
such, for instance, as Donati's comet, which may be
expected to return to the sun's neighbourhood in the
course of about two thousand years.

Lastly, there are the comets whose motions indi-
cate a path not re-entering into itself. These are of
two orders: those which retreat from the sun on a
path tending with continual increase of distance to
become more and more nearly parallel to the path by
which they had approached him; and those whose re-
treating path carries them divergingly away so that
they retreat towards a different part of the heavens
from that whence they arrived. Technically, the two
orders are those of comets pursuing (i.) parabolic and
(ii.) hyperbolic paths. In reality, however, we may
dismiss the parabolic path as never actually followed by
any comet, any more than a truly circular path is ever
actually followed by any planet. We may take it for
granted that any comet which seems to follow a
parabolic path really follows either an enormously
elongated oval path, and so belongs to our second
class; or a path carrying it for ever away into outer
space, and *nearly* in the direction from which it had
arrived, but not *exactly*. A comet's path could only
have the true parabolic form by a perfect marvel of
coincidence; and in point of fact if a comet could by

some amazing chance approach our sun on such a path the very least of the multitudinous disturbing attractions to which the comet would be exposed, would suffice to change the path either to the elliptic or the hyperbolic form.

And here we may pause to inquire how far the second of the three classes into which comets have thus been divided can be regarded as a class apart. Does the mere fact that a comet has a re-entering path— so that, unless perturbations affect it, the comet will travel in continual dependence on our sun—afford a sufficient reason for distinguishing the comet from others which travel on a hyperbolic path? It appears to me that this question admits of being answered in two ways. When we remember that a comet approaching our system on a slightly hyperbolic path might have that path changed into an elliptic figure by the perturbations to which the comet would be subjected during its visit, we may reasonably decide that the mere fact of a comet pursuing an elliptic path ought not to be considered a valid reason for distinguishing it from one of the hyperbolic comets. But when we consider, on the other hand, that there are comets like those of Jupiter's family which are quite distinctly separated by the nature of their paths from the hyperbolic comets, we may not unreasonably infer that some at least of those which travel on elliptic paths of great eccentricity are in reality to be classified apart from the hyperbolic comets, as having had a different origin and a different history. We might,

indeed, reverse the argument just adduced, and reason that the hyperbolic comets ought not to be classified apart from the comets of long period, because perturbations excited within the solar system might change an elongated elliptic orbit into a hyperbolic one. The point at issue is thus seen to resolve itself into the question whether we can assert that there are comets which from the earliest times (the youth of the solar system) have belonged to it (i.) with short periods and (ii.) with long periods, while (iii.) other comets have visited it from other systems. We find in fact that the attempt to classify leads in this case, as it has led in so many others (as perhaps it inevitably must lead, if properly conducted), to the question of origin.

And here perhaps the question will arise, May we not cut the Gordian knot by denying that even the comets of short period can be separated from the hyperbolic comets which visit our system from interstellar space? I am aware that the theory of comets and meteors which Schiaparelli has advanced, and which many in this country have viewed with considerable favour points to this conclusion. For according to that theory meteor-systems are groups of discrete bodies which have been drawn towards our solar system, gradually lengthening out as the process of indraught continued, and have then been compelled by the perturbations to which they have been subjected within our system, to become members of it; and as comets and meteor-systems have been found to be associated together in some mysterious way, this

theory of the introduction of meteor-systems is in reality a theory of comets. Now since some certainly among the meteor-systems have periods of moderate length, this theory of Schiaparelli's would regard the short-period comets as drawn out of the interstellar depths, while manifestly it would be absurd not to extend Schiaparelli's theory to hyperbolic comets. In fact, we know that he himself regards his theory as requiring the occasional appearance of meteors of hyperbolic path, and therefore as not merely consistent with the phenomena of hyperbolic comets, but accounting for them. Adopting his theory, then, to its fullest extent, we should regard all comets and meteors as bodies coming from the interstellar depths; for it is not easy to see how any comet or meteor-system could be so far distinguished from its fellows as to be regarded as originally a member of the solar system.

But for reasons which appear to me incontrovertible, I find it impossible to give in my adhesion to Schiaparelli's views, in the form in which he has presented them. A line ought to be carefully drawn between what has been proved and what has not been proved respecting the opinions which Schiaparelli has advanced. His most happy conception, that meteors would be found to travel in the paths of comets, has been realised, and no possible question can be raised as to the completeness of the demonstration; but it is quite otherwise with his supposition respecting the *manner* in which meteoric systems or comets have been introduced into the solar system. It not only has not been

proved that comets have been compelled by the per-
turbations of the planets to become permanent members
of the solar system, but grave doubts rest on the bare
possibility of such an event occurring.

Let it be remembered that the conditions of the
problem are purely dynamical. We know that a
comet's head obeys the laws of gravity, and whatever
peculiarities may affect the motions of the matter of
comets' tails are not by any means such as would help
to render easier the captures conceived by Schiaparelli.
Confining ourselves then to gravity, we can determine
readily in what way a comet might be captured. Take
the case of a particle travelling towards our solar
system from out of the interstellar depths under the
influence of the sun's attraction. Such a particle may
be regarded as practically approaching the sun from
an infinite distance,[1] and we know its velocity at given
distances from the sun. Thus, when at the distance of
Neptune its velocity would be 4·7 miles per second ;

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[1] The point considered is the velocity of the particle at given dis-
tances from the sun ; and the estimated velocity is appreciably the same
whether the particle be supposed to come from the distance of the nearest
star or from an infinite distance, This is easily seen from the formula

$$V^2 = v^2 \left(2 - \frac{r}{a} \right)$$

where r represents the radius of a circular orbit described with velocity
v, and V is the velocity at distance r, of a body travelling in an orbit
having mean distance a. For, regarding the earth's orbit as unity,
put

$r =$ earth's distance = unity,
$v =$ earth's velocity = 18·3.

taking a mile as the unit of length, and a second as the unit of time ;
(though we have put $r =$ unity, this does not force us to take r as our

at the distance of Uranus, 5·9 miles per second ; of Saturn, 8·3 miles ; of Jupiter, 11·3 miles; of the asteroids, from 15 to 16 miles per second ; and the velocity in crossing the distances of Mars, the Earth, Venus, and Mercury, would be 20·8 miles, 25·9, 30·3, and 41·4 miles per second respectively. Now we know that the greatest velocity which any given planet can communicate to a body approaching it under its sole influence from interstellar space, is very much less than the velocity which such planet can communicate to a body approaching it under the sun's influence in addition to its own. For the communication of velocity to a moving body is a process requiring time, and in the latter of the two cases just considered the body is for a much smaller time under the influence of the planet.[1]

unit of length, because we only require to consider the ratio in what follows). Then we have—

$$V = 18 \cdot 3 \sqrt{2} \sqrt{1 - \frac{1}{2\,a}} = 25 \cdot 9 \left\{ 1 - \frac{1}{4\,a} - \frac{1}{32\,a^2} - \&c. \right\} = 25 \cdot 9,$$

if a is made infinite. But if a be taken equal to half the distance of Alpha Centauri, say = 100,000, we have

$$V = 25 \cdot 9 - 0 \cdot 00006475 - 0 \cdot 000000000809375 - \text{smaller terms},$$

all the terms after the first being together manifestly less than 0·00007, or about 4½ inches. In other words, whereas a body approaching the sun from infinity would have a velocity of about 25·9 miles per second, a body approaching the sun from the distance of Alpha Centauri, so that its mean distance may be regarded as half the distance of that star, would have a velocity less by 4½ inches per second, a difference so small that it may be regarded as evanescent. It is a curious consideration, however, that minute though such differences are when we are merely comparing velocities, yet distances due to such differences in the enormous time-intervals which the study of comets introduces to our consideration, are to be measured by thousands of miles.

[1] The comparison is easily made in any given case. Take, for

And the velocity which a planet can communicate under any circumstances represents the velocity which, under similar circumstances, the planet can withdraw from a moving body. So that Jupiter, Saturn, Uranus, and Neptune are severally unable to deprive a particle which, drawn in by the sun's attraction, passes near to them, of more than a portion of the velocity which these planets are respectively able to communicate to a body approaching them from infinite space. Taking, for example, the case of Jupiter, we may regard 40 miles per second as a sort of negative fund from which Jupiter would have the power of drawing, to reduce

instance, the planet Jupiter, supposing it at rest, and a particle drawn towards it from an infinite distance under the combined influence of the sun and planet (the particle lying originally on the side away from the sun). We readily obtain for the velocity V of the particle just as it is reaching the surface of Jupiter the equation

$$V^2 = \frac{2\,M}{J+j} + \frac{2\,m}{j};$$

where M represents the sun's attractive influence at a unit of distance, and m Jupiter's, while J represents Jupiter's distance from the sun, and j the radius of Jupiter. For the velocity v of a particle under Jupiter's sole influence we obtain the equation $v^2 = \frac{2\,m}{j}$. Now it is easily calculated that $\frac{2\,M}{J+j} = (11\cdot3)^2$, while $\frac{2\,m}{j} = (40)^2$ nearly. Hence the velocity $V = \sqrt{(11\cdot3)^2 + (40)^2} =$ less than $41\cdot6$; while $v = 40$; so that a body approaching the sun under his sole influence would have, at Jupiter's distance, a velocity of $11\cdot3$ miles per second; one approaching Jupiter under the combined influence of the sun and planet would reach Jupiter's surface with a velocity of $41\cdot6$ miles per second; and a body approaching Jupiter under his influence alone would reach his surface with a velocity of 40 miles per second. So that Jupiter helping the sun adds a velocity of $30\cdot3$ miles per second as compared with the velocity of 40 miles per second, which he can communicate to a body approaching him from infinity.

the velocity of bodies moving from him, if Jupiter were the sole attracting influence under which such bodies had acquired their velocity; *but* in the case of bodies which have been drawn inwards by the sun's attraction, the fund is reduced, as shown in the last note, to about 30·3 miles per second. Now this might seem ample when we remember that the velocity of a body crossing the path of Jupiter under the sun's influence alone would be but 11·3 miles per second. But it is to be observed that the estimate only applies to bodies moving all but directly from Jupiter, and coming all but into contact with his surface. The power of Jupiter in this respect diminishes rapidly with distance from the surface. At a distance from Jupiter's centre equal to four times his radius, his power is already diminished one half, and this distance is far within that of even his nearest satellite. More-over, it is to be noticed that a body which moves in such sort that Jupiter exerts his most powerful retardative influence, must have moved for some time previously in such a way that Jupiter exerted nearly his most powerful accelerative influence.[1] It may be readily shown to be impossible for Jupiter to withdraw much more velocity than he had already communicated;

[1] It is manifest that a particle in approaching from without must be, in the first instance, accelerated by any planet to which it draws near, no matter what the direction may be in which the particle arrives. It may begin to be retarded, however, before it has reached the distance from the sun at which the disturbing planet is travelling. In any dis-cussion of the change of path as to position, we should need to inquire very carefully into the manner of approach ; but in the above discussion we are only inquiring into the change of velocity.

and similar remarks apply, of course, to Saturn, Uranus, and Neptune.

The application of these considerations to Schiaparelli's theory is easily perceived. In order that a particle attracted from outer space may be compelled to travel in a closed orbit around the sun, its velocity must be diminished. And this can very readily happen. But for the particle to travel in an orbit of a particular extent or mean distance, its velocity where it crosses the distance of the disturbing planet must be diminished by a certain amount ; and in dealing with Schiaparelli's theory, it is a cardinal consideration whether the observed orbits of periodic comets are such that we can admit the possibility of their resulting from any diminution of velocity which the disturbing planet could have produced. Take, for instance, the November meteors, which pass near the orbits of Uranus and the earth, and do not approach any other orbit near enough for any such effects upon the orbital motions of these bodies as we are now dealing with.[1] We may dismiss the earth from consideration at once, because our planet is far too small to modify the motions of bodies rushing past her with the velocity, nearly 26 miles per second, which the sun has communicated to bodies approaching him from interstellar space,

[1] Both Jupiter and Saturn can perturb the November meteors, and thus modify the shape and position of the meteoric orbits; but such changes, though by no means inappreciable, are utterly insignificant compared with those required to change the motion of a body approaching the sun from interstellar space into motion in an orbit like that of the November meteors.

by the time they reach the earth's distance from him.
Uranus then alone remains. Now the present velocity
of the November meteors when crossing the orbit of
Uranus amounts to about 1½ mile per second. The
velocity of a particle approaching the sun from inter-
stellar space would be nearly 6 miles per second when
at the distance of Uranus. It may be seriously ques-
tioned whether, under any circumstances whatever, a
particle crossing the track of Uranus without encoun-
tering the planet could be deprived of 4½ miles per
second of its velocity. For though Uranus can deprive
a body directly receding from him (and starting from
his surface) of a velocity of about 13 miles per second,
yet the considerations above adduced show that only a
fraction of this velocity could be abstracted from a body
moving past Uranus; and it is certain that if so large a
reduction as 4½ miles per second could be effected at all,
it would only be by a singularly close approach of the
particle to the surface of Uranus.

But setting apart the improbability that a body
arriving from interstellar space could be in this way com-
pelled to travel in the orbit of the November meteors,
the possibility of such a capture would not prove the
possibility of the capture of a flight of bodies large
enough to form that meteor system and its accompanying
comet. If the whole material of the system and its
comet had arrived in a compact body, the material
attractions of the parts of that body would be sufficient
to keep them together; whereas, in point of fact, the
November meteor-system and its comet occupy at pre-

sent a large range of space, even if the meteors be not
scattered all round the orbit (however thinly along
portions thereof).   If, on the other hand, the material
of the body were not in a compact form, the body
would be necessarily large, and a portion of it only
would be captured by Uranus.   Nay, it is not even
necessary that this should be conceded.   For though
we admitted that the whole of a large and tenuous body
not kept together by the mutual attraction of its parts
or by cohesion, might be captured, it is manifest that
different parts would be captured in different ways, and
would thenceforth travel on widely different orbits.
That a system of bodies already drawn out into an ex-
tended column, and in respect of length already resem-
bling the meteor systems we are acquainted with, could
be captured, as Schiaparelli's theory requires, and all
sent along one and the same closed orbit, is altogether
impossible.

It is to be noticed also that we gain nothing, as
respects the interpretation of comets, by adopting
Schiaparelli's hypothesis.   To assume that cometic
matter has been wandering about through interstellar
space, until the sun's attractive influence drew such
matter towards the solar system, is to explain a diffi-
culty away by advancing another still greater ; more-
over, we have not a particle of evidence in support of
the supposition.   To suppose, on the other hand, that
comets have *crossed* the interstellar spaces, coming to us
from the domain of another sun, is to remove the diffi-
culty only one step.   We know that comets pass away

from the domain of our sun to visit some other sun
after an interstellar journey of tremendous duration ;
and to suppose that comets, whether of hyperbolic or
elliptic orbit, came to us originally from the domain of
another sun, is merely to suppose that that happened to
such comets millions of years ago which we know to be
happening to other comets at this present day, but not
by any means to explain the nature of comets or their
origin. We know that many comets leaving our
system to visit others had not their origin within our
system ; and we cannot assume as possible or even pro-
bable that any comet had its origin within the domain
of another sun than ours, unless we assume as possible
or probable that some among the comets leaving our
own sun had their origin within our sun's domain.

Thus, then, we have been led to the conclusion that
whether we adopt, with Schiaparelli and others, the
theory that comets with meteoric systems can be drawn
into the solar domain, or regard such an event as of
very infrequent occurrence, we still find that the origin
of comets must be looked for within solar systems ; or
rather, since we cannot claim to trace back comets any
more than planets or suns, to their actual origin, we
may say that at an early period of their existence comets
belonged to the solar system. The system has had no
more occasion, so to speak, to borrow comets from other
systems—that is, from other suns—than these have had
to borrow comets from it and from each other.

We decide, then, that comets may certainly be
classified into those which have belonged to our solar

system from the earliest period of their history, those
which visit it from without, and pass away to other suns,
and an intermediate class consisting of those which having
visited it from without have been constrained, by per-
turbations affecting them within it, to become attached
permanently to its domain. We may note also that as
there are comets now belonging to our solar system
which originally belonged to other solar systems, so pro-
bably many comets originally belonging to our solar
system are now either attending on other suns or
wandering through the star-depths from sun to sun.

It has been from viewing the matter in this way, re-
cognising the almost decisive evidence that comets have
from earliest times been members of our solar system,
that I have been led to inquire into the possibility that
some comets may have been expelled from the sun, and
that others—those, namely, which seem attached to the
orbits of the giant planets—may have been expelled from
those planets when in their former sun-like condition.
The evidence to show that there is an adequate expulsive
power in the sun is striking, and we may reasonably
infer that the small suns formerly dependent upon him
had a similar power. The motions of the members of
the comet-families of Jupiter, Saturn, Uranus, and
Neptune, accord far better, too, with this theory than
with Schiaparelli's.

It is to be noticed, however, in conclusion, that we
may also not unreasonably admit the possibility that
comets may be, as it were, the shreds and fragments left
from the making of our solar system and of others, since

the sun and planets in their former nebulous condition and expanded forms would have had a power of capturing these wandering shreds which at present they no longer possess.

THE SUN A BUBBLE.

AN American astronomer of great eminence has recently suggested a very startling theory respecting the Sun, presenting that orb to our contemplation as, literally, a mere bubble, though a splendid one and of stupendous dimensions. If this theory were only advanced as a speculation, a crude notion as to what might be, I should not care to discuss it in these pages. But the hypothesis has been based on a very careful discussion of facts, and affords on the whole a readier explanation of certain observed appearances than any other which has been suggested. I propose, therefore, briefly to describe the phenomena on which the theory is founded, and then to sketch the theory itself, and some of the most remarkable consequences which must be accepted along with it, should it be admitted.

But first, I shall present some of the ideas which very eminent astronomers have entertained respecting the condition of that glowing surface which astronomers call the solar photosphere. It will be seen that the bubble theory of the sun has been far surpassed in

audacity by former speculations respecting the great
central luminary of our system.

Sir W. Herschel, during the whole course of his ob-
servations of the sun, proceeded on the assumption,
which perhaps appears a natural one, that the sun has
a solid globe around which lies an atmosphere of a com-
plex nature. I shall presently describe his strange
ideas respecting the nature of the solar globe ; but it
will be well to quote first his views as to the atmosphere
of the sun, and the analogies he recognised between the
sun's atmosphere and the earth's. 'The earth,' he said,
in a passage explaining his view as to the solar spots,
'is surrounded by an atmosphere composed of various
elastic fluids.   The sun also has its atmosphere, and if
some of the fluids which enter into its composition
should be of a shining brilliancy, while others are nearly
transparent, any temporary cause which may remove
the lucid fluid will permit us to see the body of the sun
through the transparent ones.   If an observer were
placed on the moon he would see the solid body of our
earth only in those places where the transparent fluids
of the atmosphere would permit him.   In others the
opaque vapours would reflect the light of the sun with-
out permitting his view to penetrate to the surface of
our globe.   He would probably also find that our planet
had occasionally some shining fluids in its atmosphere,
as, not unlikely, some of our northern lights might at-
tract his notice, if they happened in the unenlightened
part of the earth, and were seen by him in his long dark
night.'   He goes on to show how the various pheno-

mena of sun spots can be explained by the theory that they are due to the occasional and temporary removal of the shining atmosphere from parts of the sun. 'In the year 1791,' he proceeds, 'I examined a large spot in the sun, and found it evidently depressed below the level of the surface ; about the third part was a broad margin or plain of considerable extent, less bright than the sun, and also lower than its surface. This plain seemed to rise, with shelving sides, up to the place where it joined the level of the surface. How very ill would this agree with the old ideas of solid bodies, bobbing up and down in a fiery liquid, with the smoke of volcanoes, or scum upon an ocean ; and how easily is it explained upon our foregoing theory. The removal of the shining atmosphere, which permits us to see the sun, must naturally be attended with a gradual diminution on its borders. An instance of a similar kind we have daily before us, when, through an opening of a cloud, we see the sky, which generally is attended by a surrounding haziness of some short extent.'

He was led by considerations such as these to conceive that the real body of the sun is neither illuminated nor heated to any remarkable degree, and may, in fact, be habitable. 'The sun, viewed in this light,' he said, 'appears to be nothing else than a very eminent, large, and lucid planet, evidently the first, or, in strictness of speaking, the only primary one of our system, all others being truly secondary to it. Its similarity to the other globes of the solar system with regard to its solidity, its atmosphere, and its diversified surface ; the rotation

upon its axis, and the fall of heavy bodies, lead us on
to suppose that it is most probably also inhabited, like
the rest of the planets, by beings whose organs are
adapted to the peculiar circumstances of that vast globe.
Whatever fanciful poets may say in making the sun
the abode of blessed spirits, or angry moralists devise in
pointing it out as a fit place for the punishment of the
wicked, it does not appear that they had any other
foundations than mere opinion and vague surmise; but
now I think myself authorised, *upon astronomical
principles,* to propose the sun as an inhabitable world,
and am persuaded that my observations, and the con-
clusions I have drawn from them, are fully sufficient to
answer every objection that may be made against it.'

Before passing from the views of the greatest obser-
vational astronomer that ever lived, I shall venture to
quote yet another passage, to show on what feeble
arguments he was content to rely, when this favourite
theory of his was in question. He pictures to himself
and his readers how the inhabitants of our moon, and
of the moons circling around Jupiter, Saturn, and
Uranus, considering the offices discharged by those
planets, might be led to regard their primaries as
' mere attractive centres, to direct their revolutions,
and to supply them with reflected light in the absence
of direct illumination.' ' Ought we not,' he proceeds
seriously to demand, ' to condemn their ignorance as
proceeding from want of attention and proper reflection?
It is very true that the earth and those other planets
that have satellites about them, perform all the offices

that have been named for the inhabitants of these little globes; but to us who live upon one of these planets, their reasonings cannot but appear very defective, when we see what a magnificent dwelling-place the earth affords to numberless intelligent beings. These considerations ought to make the inhabitants of the planets wiser than we have supposed those of their satellites to be. We surely ought not, like them, to say ' The sun ' (that immense globe, whose body would much more than fill the whole orbit of the moon) ' is merely an attractive centre to us.' From experience we can affirm that the performance of the most salutary offices to inferior planets, is not inconsistent with the dignity of superior purposes ; and in consequence of such analogical reasonings, assisted by telescopic views, which plainly favour the same opinion, we need not hesitate to admit that the sun is richly stored with inhabitants.'

Sir John Herschel went far beyond his father, however, in dealing with the question of the sun's habitability. He adopted a totally different view. Admitting the possible coolness of the real solar globe, and the consequent possibility of the existence of ordinary forms of life upon it, he nevertheless preferred to regard the true inhabitants of the sun, not simply as capable of bearing an intense heat and light, but *as themselves emitting the chief part of the light and heat which we receive from the sun !* This may appear altogether incredible, and in fact, the terms in which Sir John Herschel expressed the opinion were not quite so

definite as those which I have just used. Neverthe-
less, I believe my readers, after considering the passages
I shall quote from Sir John Herschel's statement of his
views, will perceive that there can be very little doubt
as to his real opinion.

The surface of the sun, when examined with very
powerful telescopes, shows a multitude of bright granu-
lations, which, according to Nasmyth, are due to the
existence of very bright objects shaped like willow-leaves.
I do not here discuss the question whether these solar
willow-leaves have a real existence or not. Suffice it
that the evidence on the subject appeared to Sir John
Herschel to be demonstrative. 'The leaves or scales,'
he said, 'are not arranged in any order (as those of a
butterfly's wings are) but lie crossing in all directions
like what are called spills in the game of spillikins;
except at the borders of a spot, where they point, for
the most part inwards, towards the middle of the spot,
presenting much the sort of appearance that the small
leaves of some water-plants or sea-weeds do at the edge
of a deep hole of clear water. The exceedingly definite
shape of these objects; their exact similarity one to
another; and the way in which they lie athwart and
across each other (except where they form a sort of
bridge across a spot, in which case they seem to affect a
common direction, that namely of the bridge itself),
all these characters seem quite repugnant to the notion
of their being of a vaporous, a cloudy, or a fluid
nature. Nothing remains but to consider them as
separate and independent sheets, flakes, or scales,

having some sort of solidity. And these flakes, be they what they may, and whatever may be said of the dashing of meteoric stones into the sun's atmosphere &c., are evidently the immediate sources of the solar light and heat, by whatever mechanism or whatever processes they may be enabled to develope, and as it were elaborate, these elements from the bosom of the non-luminous fluid in which they appear to float. Looked at in this point of view, we cannot refuse to regard them as *organisms* of some peculiar and amazing kind ; and though it may appear too daring to speak of such organisations as partaking of the nature of life, yet we do know that vital action is competent to develope at once heat and light and electricity. These wonderful objects have been seen by others than Mr. Nasmyth, so that there is no room to doubt of their reality. To be seen at all, however, even with the highest magnifying powers our telescopes will bear when applied to the sun, they can hardly be less than a thousand miles in length, and two or three hundred in breadth.'

It is not a little singular that the two Herschels, among the ablest reasoners on observed facts, and both highly distinguished for observational skill, should have advanced theories so fanciful as the two I have quoted above. On no other evidence than the fact that the sun, like the earth, is a rotating globe, the elder Herschel was prepared, I will not say to overlook the intense light and heat of the solar orb, but to invent a protecting envelope, of a nature utterly unlike that of any material known to men of science, whereby the solar

inhabitants might be protected from the sun's fiery rays; while the younger Herschel, accepting confidently the 'solar willow-leaves' (much doubted by other astronomers) was prepared to regard them as organisms whose *vitality* supplies the light and heat emitted by the sun ! When theories so startling have been maintained by the acknowledged chiefs of modern astronomy, we may be content to regard without much surprise the theory, strange though it seems at a first view, that the sun is a gigantic bubble.

But I believe that I shall be able to show that the bubble theory has very strong evidence in its favour. Let us first consider the facts which suggested it.

Very soon after Dr. Huggins [1] had devised a method by which the coloured prominences of the sun could be studied without the aid of a total solar eclipse, astronomers discovered that in many cases the red prominences result from veritable solar eruptions. Some prominences, indeed, are obviously in a condition of comparative quiescence, floating (as it were) like clouds in the solar atmosphere, and either remaining unchanged for hours or even for days, or else undergoing only very gradual processes of alteration. But there are others which are manifestly true *jets*. It is not merely that the shape of these prominences indicates unmistakably that the matter composing them has been ejected with great violence from the sun's interior, but several have

[1] The reference above is to the first detailed statement of the method by which the prominences were to be seen without eclipse, such statement bearing date February 1868, or six months before the method was first successfully applied.

been watched during the actual process of ejection. They have been seen to rise to a great height and then either to subside slowly towards the region whence they have been ejected, or else to bend over like the curved jet of a fountain, so descending until a complete arch of red matter has been formed.

Accordingly, we find that Zöllner, Respighi, Secchi, and others who have studied the sun, have agreed in recognising the action of solar eruptive forces in the production of the jet-shaped prominences.

But the most striking evidence of the energy of the sun's eruptive forces was obtained by the astronomer to whom the Bubble Theory of the Sun is due—Professor Young, of Dartmouth College, Hanover, U.S. He was observing the edge of the sun in October 1871, having his telescope (armed with a powerful spectroscope) directed upon a long low-lying band of solar clouds. I say low-lying, but in point of fact the upper side of the cloud-layer was fully fifty thousand miles above the sun's surface, the lower side being not less than twenty thousand miles above that surface. The cloud-layer was about four hundred thousand miles in length. Professor Young was called away from his telescopic work for half an hour at a somewhat interesting epoch, for he had noticed that a bright rounded cloud was rapidly forming beneath the larger and quieter cloud-layer. In less than half an hour he returned, however; and then, to his amazement, he found that the great cloud had been literally scattered into fragments by an explosion from beneath. The small rounded cloud had

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changed in shape, as if the explosion had taken place *through* it, and all that remained of the large cloud was a stream of ascending fragments, averaging about three thousand miles in length and about three hundred in breadth.   Professor Young watched the ascent of these fragments (each of which, be it noted, had a surface largely exceeding that of the British Isles), and he found that before vanishing (as by cooling) they reached a height of about two hundred and ten thousand miles. Moreover, he timed their ascent, and from his time-measurements I have been able to demonstrate the surprising fact that the outrushing matter by which the great cloud had been rent to shreds, must have crossed the sun's surface at a rate of *at least* five .hundred miles per second !

Now no explosion can occur where there has been no repression.   When a volcano, for example, gives vent to some great eruption, the energy of the eruption is due to and corresponds with the extent of the repression which had been exerted on the imprisoned gases up to the moment of eruption.   When a bullet is fired from a gun, the velocity of its flight depends on the completeness with which before and during the passage of the bullet along the barrel, the escape of the gases resulting from the firing of the gunpowder has been prevented. And although a quantity of loose gunpowder can, in a sense, explode in the open air, yet not only are the effects of explosion altogether less marked than where the exploding matter has been confined, but the explosion takes place in no definite direction, but all around the

place where fire had been applied. In order that matter may be propelled along some particular path there must, before explosion takes place, be an enclosing substance of some sort, the yielding of which at a particular point determines the direction in which the outrushing matter proceeds.

Accordingly, both Zöllner and Respighi, in adopting the general theory that the jet prominences are phenomena of eruption, although they held different opinions as to the cause of eruption, agreed in maintaining that the eruptions must take place through some substance forming a sort of solar crust. Zöllner held that the eruptions are akin to terrestrial volcanic outbursts, while Respighi considered that some kind of electrical action was in question ; but neither astronomer doubted that the eruptions sprung from beneath a compact solid or liquid surface.

But there is one great difficulty in the assumption that the sun has a solid or liquid nucleus. The sun is a body whose density is very small by comparison with the earth's and still more by comparison with the density we should be led to expect from the consideration of the enormous gravitating and compressive energy of the sun's globe regarded as a whole. It may serve to give an idea of this energy to mention the following circumstance :—If an atmosphere constituted like ours surrounded the sun (which, for the moment, I regard as a cool body), this atmosphere, instead of doubling in density with about $3\frac{1}{2}$ miles of descent as happens with ours, would double some twenty-seven times

in that short distance, so that if at the sun's actual
surface the pressure were the same as that of the air at
our sea-level, then at a depth of $3\frac{1}{2}$ miles (and many of
the sun's spots show a depth of two or three thousand
miles) the pressure would be increased more than six
million times, under which enormous action the air
would beyond question be solidified. If we could sup-
pose that the air were not solidified, then we should
have to assume that it became compressed to a density
exceeding that of our air more than six million times
—that is exceeding the density of platinum about four
hundred times.

Now the actual density of the sun is but about one-
fourth the density of the earth, and is very little greater
than the density of water. Remembering that at the
sun's tremendous heat, vapours and gases would remain
as such at a pressure very far exceeding that to which
we can subject any gas, and probably when so compressed
as to exceed water in density, it is clear that we must
regard the sun as in the main a gaseous body. It can-
not possibly have a large solid or liquid nucleus,
whatever opinion we may form as to its having a solid
or liquid crust; for if it had such a nucleus, it would
be a much more massive body than we know it to be.
As we see, moreover, that it *must* have a solid or liquid
crust, we may fairly dismiss the idea that it has any
solid or liquid nucleus *at all.*

But there is a great difficulty in understanding how
a globe like the sun, not only glowing throughout with
the intensity of its inherent heat, but also manifestly

the scene of tremendous processes of internal disturbance, can have a crust (in the ordinary acceptance of the term) encircling its vaporous interior. The phenomena presented by the spots show us that the forces acting from within are competent to burst their way through any existing solar crust; and any ordinary crust would be reduced to fragments under the action of such forces. Moreover, it is not easy to see how a crust thus readily rent asunder and tossed on one side could act the part which the solar enclosing shell or skin certainly does perform, let its nature be what it may. The exceeding definiteness of direction recognised in the jets I have spoken of above, is sufficient to show that the crust bears sway, so to speak, over the internal gaseous nucleus, and that the gases forming this nucleus, though they escape, yet owe the energy of their outrush to the action of the enclosing shell.

The theory advanced by Professor Young seems exactly suited to meet the difficulties here indicated, and to account for those more prominent solar phenomena with which alone at present astronomers can hope to deal successfully.

He considers that the sun has no permanent crust, nor in fact any envelope which can in the ordinary sense of the term be regarded as a crust at all. But inasmuch as the vaporous globe of the sun is in the presence of what Sir John Herschel has called 'the cold of space,' a process necessarily takes place over its whole outer surface corresponding to the formation of clouds in our skies, when the vapour of water has risen to such a

height as to be condensed into the form of visible cloud. The vapours of the sun's globe consist in the main, we know, of the metallic elements, and these metallic vapours would condense into clouds composed of minute globules (or perhaps vesicles) of fluid metal. But such clouds would not usually remain in the simple cloud-form. They would be continually gathering with a rapidity of formation incomparably exceeding that which we recognise in our summer clouds, even when a great storm is approaching. They would become rain-clouds, the rain which fell from them consisting simply of molten metals. More and more heavy would this metallic rain become as it descended, even as our own rains are heavier at low levels than at considerable heights. Quite low down, and when approaching the region where the intense heat of the sun's interior would revaporise them, the metallic rains would descend in perfect sheets, forming a nearly continuous liquid envelope.

It will be well, however, to give Professor Young's own account of the theory, not only because it is always desirable in presenting views of the kind to avoid the risk of false interpretation, but because in the present instance the subject is one of so stupendous a nature, and surrounded by such great difficulty, that the reader will do well to examine the new theory in more than one aspect: ' The eruptions which are all the time occurring on the sun's surface,' says Professor Young, ' almost compel the supposition that there is a crust of some kind which restrains the imprisoned gases and through which they force their way with great violence. This crust

may consist of a more or less continuous sheet of rain, not of water, of course, but of materials whose vapours are shown by means of the spectroscope to exist in the solar atmosphere, and whose condensation and combinations are supposed to, furnish the solar heat. The continuous outflow of the solar heat is equivalent to the supply that would be developed by the condensation from steam to water of a layer about five feet thick over the whole surface of the sun per minute. As this tremendous rain descends, the velocity of the falling drops would be retarded by the resistance of the denser gases underneath, the drops would coalesce until continuous sheets would be formed, and the sheets would unite and form a sort of bottomless ocean resting upon the compressed vapours beneath, and pierced by innumerable ascending jets and bubbles. It would have nearly a constant depth in thickness, because it would re-evaporate at the bottom nearly as fast as it would grow by the descending rains above, though probably the thickness of this sheet would continually increase at some slow rate, and its whole diameter diminish. *In other words, the sun, according to this view, is a gigantic bubble whose walls are gradually thickening and its diameter diminishing at a rate determined by its loss of heat. It differs, however, from ordinary bubbles in the fact that its skin is constantly penetrated by blasts and jets from within.'*

Professor Young proceeds to remark, that ' the hypothesis leaves the question of the solar spots untouched, but is consistent with either of those most in vogue at

present.' Here, however, we have to note an interest-
ing circumstance tending to show that Professor Young's
theory is one which accords better than any other with
the phenomena presented by the surface of the sun.
Unknown to Professor Young [1] a theory not unlike his
was suggested four or five years ago by Mr. Stoney,
F.R.S., especially to explain the features presented by
the solar spots. After carefully examining the evidence,
Stoney was led to the conclusion, that the brightest
parts of the sun (the bright granules) are regions where
there are solar clouds and solar showers, the less bright
parts, on which the granules are seen as on a back-
ground, being regions where there are clouds but no
showers, and the penumbral parts of the spots being
regions where there are showers without clouds, that is
where we are looking at the edge of a shower.

In fact, if we consider those features of the solar
heat which have been regarded as most characteristic as
well as most difficult to explain, we shall find reason for
considering Professor Young's theory as affording a very
satisfactory explanation of the observed appearances.
It has always been regarded as a very remarkable cir-
cumstance that the outlines of sun-spots are well defined

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[1] Professor Young communicated to me a sketch of his theory
several weeks before he published it, inviting comments and asking par-
ticularly whether any similar theory had been previously enunciated.
A great pressure of engagements prevented me from replying at
the time to this letter, otherwise the published statement of the theory
would have contained a reference to the facts mentioned in what follows.
In any case, however, it is manifest that the views of Professor Young
and of Mr. Stoney are independent of each other, being devised in ex-
planation of two wholly distinct sets of circumstances.

not only on the inside, where the dark central part of the spot is, but also on the outside where the spot adjoins on the bright surface of the sun. But this peculiarity is explained at once, if we regard the solar shell-envelope as consisting of a very bright outer layer of clouds, from which metallic rains are falling. The edge of the clouds would then define the outside of the spot's fringe-like border, while the lower limits of the shower would define the inside. It is true that this explanation assumes that the lower limit of the showers falling all round a spot lie closer than the upper; but this would naturally happen if, as is suggested by many circumstances, a spot is a scene where there is a cyclonic downrush of matter from without; for the whirling vapours would sway the upper parts of the downfalling streams more effectively than the lower parts, which parts would therefore tend inwards towards the spot's central region.

It will probably occur to the reader that if heavy solar showers fell in this particular way, then, unless the showers were perfectly continuous (a most improbable contingency) the edges of the showers thus brought into view should show streaks radiating from the direction of the spot's centre. To explain my meaning more clearly, suppose a large region of the earth to be covered by rain-clouds from which showers are falling; then suppose a circular part of the cloud-covering removed, and that the rain falling all around this circular space slopes inward towards the middle of the space; now suppose a balloonist to ascend from the middle of the circular space until he is high above the

level of the cloud-layer ; then he would see below him a great opening in the cloud-layer (white in the sunlight, which would be shining on its outside), and he would see all round the opening and within it the streams of falling rain, forming, as it were, a fringe within the circular gap ; and it is manifest that this fringe would show streaks in the direction of the falling rain-streams, that direction as seen by the balloonist appearing to be radial with respect to the circular openings. Now it has long been noted as one of the most remarkable features of the solar spots that their penumbral fringes *are* streaked precisely in this manner.

But again, it will be seen that if falling solar showers were thus thrust outwards at their upper edges, then— since lines drawn towards a centre lie closer as the centre is approached, the penumbra of a spot ought to be brighter at its inner edge than at its outer.   The difference would be rendered all the more remarkable because the showers would grow heavier as they descended, according to the law observed in our rain-showers.   Now here, again, it is a noteworthy circumstance that long before the bubble theory of the sun had been invented astronomers had recognised the fact that the penumbral fringe of a spot is markedly darker on the outside than on the inside.   The observation has been made in such a way as to preclude the possibility that contrast alone would account for the phenomenon.   Thus a second, and most remarkable feature of sun-spots, finds its explanation in the new theory.   I venture, indeed, to say with some confidence that the appearance in ques-

tion suffices to throw serious doubts upon all other theories which have hitherto been propounded in explanation of sun-spot phenomena. I do not say that the bubble theory can be regarded as demonstrated on the strength of this simple fact; but I do assert that no theory hitherto put forward has given any account whatever of the peculiarity in question.

It is manifest, however, that Professor Young's theory gives no explanation of the origin of sun-spots, nor does the theory throw any light whatever on that perplexing subject. Nevertheless, it is impossible to consider the condition of the sun, as presented by the startling theory before us, without being led to re-examine the questions suggested by what we have learned respecting sun-spots. We see confirmed by the theory, the view to which astronomers had for some time been led, that spots are produced by action exerted from without. We perceive reasons for believing that this action is one of great energy, its energy being probably in the main dynamical. It is true that the darkness of a spot must be explained by physical considerations depending on the laws of heat and light, and that chemical relations must be taken into account in dealing with the subject. But we seem to recognise clear evidence of the actual thrusting on one side of solar clouds with their downpour of metallic rain, where spots are formed. Apart from the considerations relating to the penumbral fringe of a spot, there is a manifest heaping up of the solar cloud-layers all round a spot, where the bright and elevated regions called

faculæ are seen. Besides, many spots indicate by their shape and changes of shape the action of most energetic forces, breaking up and thrusting apart, as it were, the masses of clouds which form the light-giving surface of the sun.

Now the various theories, which have been formed to account for the periodic recurrence of spot-frequency, have been based on influences supposed to be exerted in some mysterious manner by the planets. In particular, Jupiter has been held responsible for the great spot-period of about eleven years. Jupiter's period of revolution around the sun being about eleven years and ten months, it has been inferred that he regulates this period of spot-frequency ; and a comparison has been made between his supposed action in this respect and the apparent connection existing between our moon's motions and the recurrence of terrestrial volcanic action. It is manifest that the explanation (if such it can be called) thus indicated would correspond with a theory presenting sun-spots as caused by solar forces acting from within outwards, but would by no means accord with a theory indicating as the *source* [1] of solar spots an action exerted from without the solar orb. More-over, we cannot readily overlook the circumstance that the eleven-year spot-period does not accord exactly with Jupiter's period of revolution. In consequence of this want of agreement, we have not to go far back to find

[1] I emphasize the word 'source,' because whatever opinion may be formed as to the origin of sun-spots, no doubts can be entertained respecting the action of explosive solar forces.

periods when spots have been very numerous, corresponding with the time when Jupiter has been (i.) at his nearest to the sun, (ii.) farthest from the sun, and (iii.) at his mean distance. This appears to render altogether untenable the theory that there is any connection whatever between Jupiter's distance from the sun and the appearance of spots upon the sun's surface. And if we give up the theory that Jupiter influences the sun in this manner, it seems impossible to believe in planetary influence at all. So that we may regard ourselves as free to search for other causes, and especially for the possible existence of matter reaching the sun from time to time from without and so producing those openings.

Thus viewing the matter, one might be led to suspect the existence of some as yet undetected comet with its train of exceptionally large meteoric masses, travelling in a period of about eleven years around the sun, and having its place of nearest approach to that orb so close to the solar surface that when the main flight is passing the stragglers fall upon the sun's surface. But then there is this difficulty, that the spots appear always on *two* zones of the sun's surface, corresponding in a general sense to the temperate zones on the surface of the earth, and though it would be easy to account for one such zone by the suggested comet theory, the existence of two is not so readily accounted for.

And yet though no single comet can be accepted in explanation of the observed facts, there are some circumstances which, so soon as the general idea of cometic

influence has been mooted, attract our attention as favouring that theory. For example, if we ascribed the sun's spots to comets, we should require that many comets should have paths carrying them very close to the sun's surface; and though few such comets have been detected, yet the law observed in the paths of discovered comets indicates that if we only had an equal chance of detecting comets which passed very near to the sun, they would be found to be very numerous indeed. It has been shown that, if a model of the solar system were constructed and a material particle were set to indicate that point of each cometic path which lies nearest to the sun, the density with which such particles would be aggregated would be found to increase rapidly in approaching the sun.

Again, since there are two zones of sun-spots, we should expect to find the cometic paths showing an average slant to the level of the sun's equator, according with the corresponding slant in the case of lines drawn from the spot-zones to the centre of the sun's globe. Such a tendency has been discovered, though the assigned slant of the cometic orbits is somewhat greater than the theory requires. Let me be permitted to quote, notwithstanding the technicality of its terms, a passage from Dunkin's excellent Appendix to Lardner's 'Astronomy,' in which this relation is stated: ' There are evident indications of a tendency of the planes of the cometary orbits to collect around a plane whose inclination to the plane of the ecliptic is forty-five degrees ; or if a cone be imagined to be formed having

a semi-angle of forty-five degrees, and its axis at right angles to the plane of the ecliptic, the planes of the cometary orbits betray a tendency to take the position of tangent-planes to the surface of such a cone.' I beg those of my readers who eschew cones, semi-angles, and tangent planes, to trust in my assurance that the sentence just quoted bears the meaning I have assigned to it. So far then the observed relations among cometic orbits seem to accord with the idea that the meteoric stragglers following on the track of comets may be in some way the cause of solar spots.

But we might also expect, if this theory were the true one, that some great comet which had approached the sun very nearly would give evidence in favour of the theory. For we could hardly but suppose that such a comet would be followed by very large meteoric attendants, and we might expect to find some one or other of these not passing like the parent comet quite clear of the sun, and accordingly occasioning (if the theory be true) a great spot. Such evidence would be particularly striking if it occurred at a time almost midway between two epochs when spots had been very numerous. Now, a comet once appeared which made a singularly near approach to the sun's surface. This was the comet of 1843, which Sir John Herschel thus graphically describes : ' Many, I dare say, remember its immense tail, which stretched half-way across the sky after sunset in March of that year. But its head as we here saw it, was not worthy of such a tail. Farther south however, it was seen in great splendour. I possess

a picture by Professor Piazzi Smyth, Astronomer-Royal of Scotland, of its appearance at the Cape of Good Hope, which represents it with an immensely long, brilliant, but very slender and *forked* tail.   Of all the comets on record, that approached nearest the sun.   Indeed, it was at first supposed that it had actually grazed the sun's surface, but it proved to have just missed by an interval of not more than 80,000 miles, about a third of the distance of the moon from the earth, which (in such a matter) is a very close shave indeed to get clear off.   There seems very considerable reason to believe that this comet has figured as a great comet on many occasions in history ; and especially in the year 1668, when just such a comet, with the same remarkable peculiarity, of a comparatively feeble head and an immense train, was seen at the same season of the year, and in the very same situation among the stars.   Thirty-five years has been assigned with considerable probability as its period of return, but it cannot be regarded as quite certain.'   Now, this remarkable comet having passed thus close to the sun, in the year 1843, which was very nearly the time of fewest spots,[1] afforded precisely such an opportunity for testing the comet theory of sun-spots as I have indicated above.   This would be a time

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[1] This will be manifest from the following numbers, indicating how many new spots were observed in the years between 1836 and 1849 :— In 1836, 272; in 1837, 333; in 1838, 282; in 1839, 162; in 1840, 152; in 1841, 102; in 1842, 68; in 1843, 34; in 1844, 52; in 1845, 114; in 1846, 157; in 1847, 257; in 1848, 330; and in 1849, 238. We thus see that 1837 and 1848 were years of greatest spot-frequency, while 1843 was a year of least spot-frequency.

when we should expect no large spot to make its ap-
pearance, for it has been observed that the larger spots
occur at or near the time when spots are most numerous.
But Professor Kirkwood (of Bloomington, Indiana, U.S.)
has called attention to the fact, that 'one of the largest
and most remarkable spots ever seen on the sun's disc
appeared in June 1843, and continued visible to the
naked eye for seven or eight days. The diameter of this
spot was, according to Schwabe, 74,000 miles, so that
its area was many times greater than that of the earth's
surface.' 'It would seem,' he proceeds, commenting on
the facts mentioned above, 'that the formation of this
extraordinary spot was an anomaly, and that its origin
ought not to be looked for in the *general* cause of the
spots of Schwabe's cycle.' He then describes, as hav-
ing a possible bearing on the question, the wonderful
phenomenon observed simultaneously by Carrington
at Redhill and Hodgson at Highgate, in 1859, when
two intensely luminous bodies seemed to burst into view
on the sun's surface, which moved side by side for
about 35,000 miles in five minutes, first increasing,
then diminishing in brightness, then fading away.
'The opinion has been expressed by more than one as-
tronomer,' he proceeds, 'that this phenomenon was
produced by the fall of meteoric matter upon the sun's
surface. Now the fact may be worthy of note that the
comet of 1843 actually grazed the sun's atmosphere
about three months before the appearance of the great
sun-spot of the same year. Had it approached but
little nearer the resistance of the atmosphere would

probably have brought its entire mass to the solar sur-
face. Even at its actual distance it must have produced
considerable atmospheric disturbance. But the recent
discovery that a number of comets are associated with
meteoric matter, travelling in nearly the same orbits,
suggests the inquiry whether an enormous meteorite
following in the comet's train, and having a somewhat
less perihelion distance, may not have been precipitated
upon the sun, thus producing the great disturbance ob-
served so shortly after the comet's perihelion passage.'

We will not further pursue this theme, however,
interesting though the considerations it suggests may
be. We have, indeed, been led somewhat far away
from the bubble theory of the sun with which we
began. But after all, in the present state of our know-
ledge of the great central luminary of the system, we
can hardly be too ready on the one hand to look
around for all side lights which may perchance help
us to see our way towards the truth, or too watchful
on the other hand lest we be led astray. So that I
need offer no excuse for directing attention to the asso-
ciation which may possibly exist between solar and
cometic phenomena, though I must at the same time
caution the reader against the supposition that such
an association can be regarded as in any sense demon-
strated.

It cannot, indeed, be too often insisted upon that
in discussing so stupendous an object as our sun, the
scene of processes so marvellous, and the centre of
activities so tremendous, we must not expect to find

simple theories of its constitution or of the changes
which it is undergoing. It is altogether a mistake for
the students of astronomy to range themselves on this
side or on that when diverse solar theories are advanced,
as though necessarily the truth must lie on one side or
the other. Whether the sun-spots are phenomena of
indraught or of outrush; whether the corona is due
to expulsive forces, to perpetual solar auroras, or to
meteoric systems in the sun's neighbourhood; whether
the sun's photosphere is solid, liquid, or gaseous;
whether his heat is due to meteoric downpour, to
the gradual contraction of his globe, or to chemical
changes: these and a hundred other such questions
may be made the subject of endless controversy, simply
because the truth does not lie altogether on one side.
Such controversy cannot but be useless in the present
state of our knowledge. It does, indeed, occasionally
happen even in dealing with solar phenomena that a
decision can be pronounced decisively between con-
tested theories, so soon as certain considerations have
been fully taken into account. A noteworthy instance
was afforded by the long-continued discussion whether
the corona is a solar appendage: a question which
really admitted of being answered definitely on the
strength of a few not very recondite mathematical
considerations, long before eclipse photography dis-
posed of it. But such cases are the exception, not
the rule. Now that we know how exceedingly com-
plicated is the structure of the sun; that processes
are taking place within his globe which are not merely

wonderful in their extent and variety, but proba-
bly for the most part quite unlike any that we are
or can ever be familiar with; when we see how the
tremendous attractive energies of the sun by which
the great gaseo-liquid mass which sways our system
is compressed towards its centre, contends continually
with mighty expulsive forces by which vast masses of
matter are visibly projected from the sun, and with
still mightier repulsive forces, whose action we see in
the phenomena of comets; when again we consider
that all the known elements probably exist in the
sun in quantities such as we can form no conception
of, and in forms with which we are unfamiliar, it is
mere folly to insist on adopting definite theories re-
specting the sun's condition. Let us remember that
in all probability we see in the sun a state of things
partially resembling what existed in our own earth
countless ages before the changes began which our
geologists find so difficult to interpret; and seeing
thus that we have a state of things removed from us
in this sense by a practical infinity of time, existing
on a globe too remote in space to be studied by any
really satisfactory methods of research, and presenting
only its glowing surface for our examination, seeing
also that although some of the forces at work there
are nominally those whose action we are acquainted
with, yet even these act on a scale which must render
their operation as utterly unlike that of the same
forces on earth as though they were forces of a totally
different nature, while lastly we cannot doubt that

forces utterly unknown to us are at work in the sun, we may well look doubtingly on the easy and simple (but contradictory) theories of the sun which are from time to time presented by students of science in this country and abroad. After many years of patient labour, we shall begin to comprehend more clearly than at present how utterly incomprehensible is the great centre of our system; for though many difficulties which now perplex us may the have been removed, each difficulty mastered will be found to have introduced others greater than itself.

(From the *Cornhill Magazine* for October 1874).

THE SUN'S SURROUNDINGS AND FUTURE ECLIPSES.

WHILE news had still to be received from some of the stations for observing the recent transit of Venus, astronomers had already turned their thoughts to another phenomenon, the observation of which may be expected to throw new light on the physical condition of the sun, and preparations were already in progress for observing the eclipse of the sun which occurred on the 6th of April, 1875. I propose to sketch the recent history and the present position of solar research, in order that the reader may understand precisely what new information astronomers hope to obtain during future eclipses. But first I shall make a few remarks on

the physical aspect of the recent observations for deter-
mining the sun's distance.   For, in point of fact, the
observations made on Venus in transit on the 9th of
December last, though primarily directed to mere
measurement, have an important bearing on our ideas
respecting the sun's condition.   On our estimate of
the sun's size and mass depends the opinion we are to ·
form respecting his power as a ruler of matter, and
respecting the duration of his existence as the light
and life of the solar system.   An error of a hair's
breadth in the position of the small disc of Venus in
one of the four-inch photographs of the sun taken
during the late transit would imply a difference in the
sun's volume exceeding myriads of times the volume
of the earth, and a corresponding difference in his
mass, while the estimated life of the sun would be
shortened or lengthened by millions of years.   It is
only necessary to consider the absolute proportions of
the sun, his mighty mass, his amazing fund of vitality,
to see how largely even minute changes in his esti-
mated distance must affect all these relations.   A globe
as large as the earth placed close to the sun's surface
would be undiscernible, save in a powerful telescope.
A globe as large as the earth, but having a surface
glowing with the intense heat of the solar surface,
would, at the sun's distance, afford but the 11,600th
part of the light and heat we receive from him.   A
globe as large as the earth, but of the same density as
the sun, and occupying his place, would possess but
the 1,250,000th part of his attractive might, and

would be utterly unfit to sway the movements of a scheme like the planetary system. Exceeding this earth on which we live so enormously in size and power, while emitting at each instant quantities of light and heat so vastly surpassing that which our earth would give out, even if every mile of her surface were caused to glow with a brightness far surpassing that of the electric light, it will readily be conceived that very moderate changes in our estimate of the sun's distance correspond to enormous changes in our estimate of his size, power, and heat. Consider, for instance, the recent modification in the estimated solar distance, from about $95\frac{1}{4}$ millions to about $91\frac{1}{2}$ millions of miles—that is, roughly, the diminution of the estimated distance by about one-thirtieth part. This corresponded to a diminution of the sun's diameter by about a thirtieth part, of his surface by about a fifteenth part, and of his volume and mass by about an eighth part. But the former estimate of the sun's mass amounted to 355,000 times the mass of the earth, so that an eighth part of this corresponded to more than 44,000 times the mass of the great globe on which we live. By this enormous amount the former estimate of the solar mass had to be reduced.

But there is yet another way of viewing the effects corresponding to changes in our ideas respecting the distance of the sun, which may be regarded as even more striking, since it relates to the sun's character as the source of all the forms of energy with which we are familiar. For, after all, mere bulk and mass count

for little.   We can even understand (without alto-
gether admiring) the rejoinder made by one to whom
an astronomer had described the vast scale of the
material creation—that after all ' this proved only that
dirt is cheap in the universe.'   But active energy, as
distinguished from the potential energy residing in
mass, is suggestive of purpose (whether correctly so or
not need not here concern us).   Regarding the sun as
the central fire of the solar system, we see that every
second of its existence corresponds to the emission of
so much heat, or, in other words, to the exhaustion of
such and such a portion of its inherent life.   Now it is
a strange thought that any change in the estimated
distance of the sun corresponds to a change in our
estimate of the heat he is momentarily pouring forth
on all sides, of the work he is performing as a mighty
and beneficent ruler of a scheme of circling worlds.
The quantity of heat emitted by the sun in every
second is so stupendous that all ordinary modes of
representing his action fail us.   It is a mere form of
words, for instance, conveying no clear ideas to the
mind, to say that in each second the sun gives out as
much heat as would be given out in the burning of
eleven thousand six hundred millions of millions of
tons of coal.   But not only is this so, but even so
slight a change as astronomers expect from the recent
observations for determining the sun's distance corre-
sponds to the increase or diminution of the estimated
outpouring of heat by an amount absolutely inconceiv-
able.   Suppose, for instance, that the estimate of the

sun's distance were increased or diminished by nearly a quarter of a million of miles, a mere nothing compared with the change which lately had to be made. This would correspond to about a four-hundredth part of the distance now regarded as probable, and would increase or diminish the estimated surface of the sun by one two-hundredth part. Now our estimate of the quantity of heat emitted by the sun corresponds precisely with our estimate of the sun's surface, so that the change supposed would correspond to the increase or diminution of the sun's momentary emission of heat by one two-hundredth part. Therefore we should have to conclude that in each second the sun gave out more heat or less heat than now supposed by the quantity of heat which would be given out by about fifty-eight millions of millions of tons of coal. Fifty-eight globes, each as large as the earth, and glowing with the same heat as the sun (mile for mile of surface), would be required to give out each second the amount of heat thus added to or taken from the solar emission in each second of time.

Another strange thought in connection with the determination of the sun's distance is this—that the farther or nearer the sun is from us the longer he will continue to perform his present functions as life-giving centre of the solar system. For in every estimate of the continuance of his reign we have to take into account the quantity of matter contained in his globe, and the extent of the region of space over which he bears supreme sway; and our estimate of his power in

both these respects depends, as we have already seen, on the views we form as to his distance.

When we add to these considerations the thought that the scale on which all the processes taking place within and around the sun's globe, the velocity with which every planet travels, as well as that with which comets and meteors approach the solar globe, the proportions of every planet in the solar system, and the distance and real splendour of every star known to us, depend on the estimate we form of the sun's distance, we see that the recent observations bear closest relation to all the most interesting physical problems with which the astronomer has to deal. Nevertheless the phenomenon to which astronomers have lately directed their attention—the recent eclipse of the sun—is one from which they hope to obtain more direct testimony respecting the physical constitution of the wonderful orb which reigns over the planetary system.

Let us turn now to the consideration of the nature and condition of the sun and his various appendages, as at present understood, in order that we may perceive what new information may be looked for during future solar eclipses. In considering the history of recent researches I shall go back over fifteen years; but I may remark at the outset that my sketch must necessarily be so slight that many important contributions to solar physics can only be touched upon, or may even perhaps be omitted altogether. In such cases no

slight is intended towards the workers, the requirements of space having alone been in question.

When the important eclipse of June 1860 was approaching, astronomers were not quite certain as to the existence of any solar matter or appendages outside the visible solar globe. Coloured objects had recently been seen surrounding the dark disc of the moon in total eclipse, like garnets round a brooch of jet, and outside these again the glory of the corona had long been recognised ; but astronomers did not agree in regarding these as belonging to the sun. Whether the evidence already available might not have been effectually and advantageously used to dispose of such doubts need not here concern us. Suffice it that amongst those who so doubted were several skilful astronomers, and pre-eminent among them M. Faye, one of the ablest mathematical astronomers of our day. The eclipse of 1860 will be always celebrated on account of the demonstration which it afforded of the nature of the ruddy flames seen round the eclipsing body of the moon. The demonstration was effected by De la Rue and Secchi, each of whom succeeded in obtaining several photographs of the total eclipse, showing the dark disc of the moon at successive stages of its passage across the prominences. Thenceforth the coloured protuberances were recognised by all astronomers as unmistakably solar appendages. And very wonderful appendages they were necessarily considered. For these ' garnets ' were now seen to be not only enor-

mously larger than the brooch round which they seemed
set—the globe of our moon—but to exceed our own
earth many times in volume. Some of those seen in
De la Rue's photographs extended more than 80,000
miles from the sun's surface; and several of them, at a
very moderate computation of their extension over the
sun's surface (of which their apparent figure gave no
direct evidence), must have occupied thousands of
times as much space as our earth's globe!

A year before this noteworthy discovery had been
made, the method of research called spectroscopic
analysis had suddenly acquired new and wonderful
powers. Kirchhoff had shown how the dark lines which
cross the rainbow-tinted streak called the solar spectrum,
speak of the presence around the solar orb of the vapours
of many elements familiar to us—iron, copper, sodium,
magnesium, hydrogen, and so on. His inference was
that the visible orb we call the sun, which astronomers
call the solar photosphere, is not only enveloped within
a dense and complex atmosphere, but that all round it,
and extending possibly even as far as the outermost
limits of the corona seen during solar eclipses, there are
masses of vapour which cut off the portions of the sun's
light corresponding to the dark lines in the spectrum.
But although the vapours around the sun thus indicate
their presence by darkening parts of the solar spec-
trum, yet beyond question they must be themselves
luminous, seeing that not only their position so close
to the sun, but the very fact that they are vaporised,
implies an intense heat. If we could take a mass of

iron (as we might take a mass of ice), melt it and then boil it (even as the melted ice might be boiled), and the vapour of iron rushed out through an orifice without being immediately condensed into metallic spray, the vapour would not be, like the vapour of water, invisible, but would glow with intensity of heat.[1] Accordingly it began to be recognised soon after Kirchhoff's great discovery, that the coloured prominences, and possibly even the corona, might be composed of those very gases whose presence Kirchhoff recognised by the dark lines in the solar spectrum.

Several years elapsed during which no fresh light was thrown on the solar surroundings. But a circumstance occurred in May 1866, which, though at first sight appearing very little connected with the study of solar physics, was destined to lead to very important results. It is curious also as one among several instances during the last thirty years or so in which the progress of astronomy has been strangely aided by lucky coincidences. The discovery of Neptune, for instance, would have been impossible but for the lucky accident that the disturbance experienced by Uranus reached its greatest amount at a time when observations had been continued long enough to give a stand-point whence

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[1] The experiment is, of course, impossible, because, under any conditions admitting of our watching the outlet whence the vapour was to pass, nothing like the requisite degree of heat could be maintained. The vapour of iron is really present in the atmosphere immediately above molten iron, but not under circumstances admitting of our testing its luminosity. Experiments in which iron is vaporised by the electrical discharge sufficiently establish the point in question however.

the mathematician might throw his line out into space till the unseen planet should be felt guiding him as it were in the true direction.[1]   The wonderful series of discoveries recently made respecting meteors and comets would have been impossible but for two or three lucky accidents by which precisely the sort of information required to complete the evidence was obtained just when it was wanted.   In the present instance it is not quite so clear that researches in solar physics would have taken a different course but for the event now to be recorded; nevertheless it is certain that this event started speculations which led directly to important discoveries.

In May 1866, a new star suddenly blazed forth in the constellation of the Northern Crown—or rather a star which had long been shining so feebly as only to be visible with telescopes of some power, acquired suddenly the brightness of a second magnitude star. Of course this interesting object was at once examined by spectroscopists both in England and abroad.   It was

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[1] This is not a supposition based merely on the probability that the search for Neptune would not have been undertaken but for the circumstance above mentioned.   If Uranus had been discovered in the middle of the present century, mathématical analysis applied to the peculiarities of the motion of Uranus, on such suppositions as Adams and Leverrier employed, would have failed to guide them to the true place of Neptune.   In fact, in one sense the eminent American mathematician, Pierce, was quite right in stating that the true Neptune is not the Neptune either of Adams or Leverrier.   Leverrier's Neptune and Adams's Neptune, though near enough together in 1846, would now be far apart; but they would be nothing like so far apart as either of those hypothetical bodies would now be from the true Neptune, which is travelling in a widely different path.

found to have a peculiar spectrum. The faint rainbow-tinted streak crossed by fine dark lines, forming the usual spectrum of a small star, was seen ; but upon this streak, as on a relatively dark background, four intensely bright lines were seen, in the place ordinarly occupied by the dark lines indicating the presence of the gas hydrogen *absorbing* the brighter light from the star's photosphere. It was manifest that hydrogen surrounded that distant sun, but that the hydrogen, instead of being relatively cool like that which surrounds our sun and other suns of the same family (as Capella, Aldebaran, &c.), was glowing with far greater heat than the sun which it enveloped. Beyond all question that sun out yonder in space—an orb which, for aught that is known, may have been as important in the scheme of creation as our own sun—had suddenly burst into flames. Its lustre when at its brightest was estimated at one hundred times its former and present brightness. The sun which had blazed out in this wonderful manner gradually lost its abnormal brightness, and has now resumed its position among the stars of the tenth magnitude.

But in the meantime a lesson taught by this star had been noted by spectroscopists. Of course there was nothing very surprising in the fact that hydrogen intensely heated should show its bright lines on the relatively dark background of a rainbow-tinted spectrum. In fact, Kirchhoff's original discovery, as interpreted by himself, implied plainly enough that this would happen. But when astronomers came to consider

this question—How much of the star's new light corre-
sponds to the intensely bright lines of its compound
spectrum, and how much to the rainbow-tinted back-
ground ?—their attention was directed to a fact very
obvious when once indicated, but the practical applica-
tion of which, if not the fact itself, had hitherto
unaccountably escaped the attention of spectroscopists.
All the light from the glowing hydrogen was concen-
trated in four lines, all the rest of the light was spread
over the ribbon of rainbow-tinted light. Now, the
greater the dispersive power of the spectroscope em-
ployed, the longer would be the ribbon of light, and
therefore the fainter, for only the same light is spread
over it in any case ; but the increase of the dispersive
power would only throw the bright lines of the hydrogen
light further apart, and would leave them as bright as
ever.   Now we need pay no further attention to this
fact in its relation to the new star, but in its relation
to the spectroscopic study of the sun it is all-important.
If the light from one source can be weakened in this
way by dispersion while the light from another source
is left unaffected, we are no longer necessarily com-
pelled, in studying the sun, to give up all hope of
recognising fainter lights which the glory of sunlight
obliterates from view.   By all ordinary methods of
observation it was manifestly hopeless, for example, to
look for the solar prominences without the aid of an
eclipse ; for any means by which the intense light of
the sun was effectively diminished obliterated the faint
light of the prominences altogether.   But here was a

means which might reduce sunlight to any desired degree and leave the prominence light unaffected, if only the prominences consist of glowing gas and so give a spectrum of bright lines. The sunlight could be spread out into a long ribbon of rainbow-coloured light and correspondingly reduced, while the bright lines belonging to the prominences would only be thrown further apart.

At this stage I find some difficulty in proceeding without hurting the susceptible feelings of one or other of the students of science who entered on this field of research. Rival claims have been advanced more or less positively—in some cases directly, in others indirectly. I have no wish to decide in favour of any of the claimants; yet if I describe the facts as they appear to us, I shall not be held guiltless by some of those who are interested. If I describe the case as resembling that of the discovery of sun-spots after the telescope had been invented, and say (as Sir J. Herschel said in that case), that the question of priority is hardly worth disputing over, I shall probably offend all those interested, as well as their friends and adherents. As the least of two evils, I shall give a brief sketch of the facts as I view them, premising that I have not the slightest feeling one way or the other as to the credit, be it greater or less, due to the contesting claimants.

It would seem that Huggins, Stone, Lockyer, and Secchi nearly simultaneously conceived the idea of applying the principle sketched above to the search for the solar prominences without the aid of an eclipse.

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Huggins points to passages in his remarks about nebulæ
which indicate his recognition of the principle so far
back as 1864. Lockyer, in October 1866, in a paper
read before the Royal Society, wrote as follows :—' See-
ing that spectrum analysis has already been applied to
the stars with such success, it is not too much to think
that an attentive and detailed spectroscopic examina-
tion of the sun's surface may bring us much knowledge
bearing on the physical constitution of that luminary.
. . . And may not the spectroscope afford us evidence
of the existence of the "red flames" which total
eclipses have revealed to us in the sun's atmosphere,
although they escape all other methods of observation
at other times ? and if so, may we not learn something
from this of the recent outburst of the star in Corona?'
Those who think the method really due to Huggins,
however, consider these remarks too vague to found a
case upon, and quote Huggins's detailed account of the
method in the Report of the Astronomical Society for
February 1868, in which he left nothing to be desired
as respects distinctness in description. 'During the
last years,' says this report, 'Mr. Huggins has made
numerous observations for the purpose of obtaining a
view, if possible, of the red prominences seen during a
solar eclipse. The invisibility of these objects at ordi-
nary times is supposed to arise from the illumination
of our atmosphere. If these bodies are gaseous, their
spectra would consist of bright lines. With a powerful
spectroscope the light reflected from our atmosphere
near the sun's edge would be greatly reduced in inten-

sity by the dispersion, while the bright lines of the
prominences, if such be present, would remain but
little diminished in brilliancy.' It is to be remarked
that Huggins himself seemed to consider Lockyer's pre-
vious statement unsatisfactory, seeing that, as editor of
Schellen's 'Spectrum Analysis,' we find him saying that
'in Mr. Lockyer's communication to the Royal Society
in October 1866, there was no statement of a method
of observation or of the principles on which the spec-
troscope might reveal the red flames.' Secchi says that
he had long had the intention of applying the method,
but was prevented by Lockyer's statement that nothing
more could be seen round the sun's edge than on the
disc itself.

The eclipse of August 1868 approached while as yet
neither Huggins, Stone, Lockyer, or Secchi had suc-
ceeded in seeing the prominence spectrum, insomuch
that a general impression prevailed that the promi-
nences do not consist of glowing gas. A more powerful
spectroscope than he had yet used was, however, being
made for Lockyer by Browning, and, for aught that is
known, this instrument would have solved the problem
of determining the nature of the prominences, but for
the fact that the eclipse of 1868 occurred in the in-
terim, and was successfully observed. For it was
during the total obscuration of the sun on that occa-
sion that the spectroscope applied to the coloured pro-
minences revealed the fact that they consist of glow-
ing gas. Colonel Tennant, Captain Herschel, MM.
Janssen and Rayet, in India, and Weiss at Aden, all

recognised three bright lines, red, orange, and blue,
while Janssen and Rayet saw other fainter lines; and
thenceforth it was an assured scientific fact that the
solar prominences are masses of glowing gas.

Then followed that episode, with the history of
which most of our readers must be familiar, recalling
in strangeness (though far inferior, of course, in in-
trinsic importance[1]) the circumstances attending the
discovery of Neptune.   Janssen during the eclipse had
noted the exceeding brilliance of the prominence-lines,
and being, no doubt, familiar with the anticipations of
Huggins, Stone, Lockyer, Secchi, and others, he recog-
nized at once the possibility of seeing those lines with-
out the aid of an eclipse.   He relates that as the sun
re-appeared, and the prominence-lines faded away, he
exclaimed, 'Je reverrai ces lignes-là en dehors des
éclipses.'   He was prevented by clouds from carrying
out his intention on that self-same day; but on the
morrow ' he was up by daybreak to await the rising of
the sun, and scarcely had the orb of day risen in full
splendour above the horizon when he succeeded in
seeing the prominences with perfect distinctness.   The
phenomena of the previous day had completely changed
their character; the distribution of the masses of gas
round the sun's edge was entirely different; and of a
great prominence,' which had formed a most conspicu-
ous feature on the preceding day, ' scarcely a trace re-
mained.   For seventeen consecutive days Janssen con-
tinued to observe and make drawings of the promi-

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[1] Simply because the discovery to which it related was assured, inde-
pendently of the race between Janssen and Lockyer for priority.

nences, proving that these gaseous masses changed
their form and position with extraordinary rapidity.'
On September 19, or a full month after he had first
seen the prominence-lines without the aid of an
eclipse, this easy-going gentleman first thought of
sending off a paper, communicating his discovery, to
the French Academy of Sciences. It arrived just
too late to anticipate an announcement addressed to
the same body by Mr. Lockyer, who, on October 16,
had succeeded in seeing the lines with the spectroscope
which Browning had made for him. Mr. Lockyer's
letter had been read about five minutes before M.
Janssen's was placed in the hands of the President of
the Academy.

Presently the new method was rendered much more
complete and effective by an arrangement, devised by
Huggins, for seeing the whole of a prominence at once,
instead of a mere line belonging to the prominence.
The difference between the original method and this
new one may be thus illustrated. Let a long straight
hole, say two inches long by about a sixteenth of an
inch in width, be cut in a card, and let a small picture,
say a *carte de visite*, be examined through this aperture
by slowly passing the card backwards and forwards over
the picture. An idea of the nature of the picture can
be formed in this way, but it would clearly be better to
have a much larger aperture cut in the card, so that
either the whole picture or a much larger portion of it
could be seen at once. Mr. Huggins showed how this
could be done by opening the jaws of the slit through
which the prominence spectrum was examined. The

wonder was that the idea had not been thought of
earlier.[1]

It was now possible to study the solar surroundings
at leisure. Not only could the structure of the ruddy
prominences be examined, but their constitution. It
was found that Grant, Secchi, and Leverrier had been
right in asserting that the prominences are but the
higher parts of an envelope of this ruddy matter entirely
surrounding the sun. Secchi had called this envelope
the sierra, but a new name was devised for it, and it is
now commonly called the *chromosphere* (somewhat as
the glowing surface of the sun might have been called
the *phosphere*, had the deviser of a name for it chanced
to be ignorant that the word should be *photosphere*).
The chief constituent gas of the prominences is hydro-
gen, but there is another gas always present, not only
in the prominences but in the sierra, which gives a
yellow-orange line as yet not identified with a charac-
teristic line of any known element. The earliest exami-
nation of the sierra, however, showed the continual
presence of several other lines, while later examination
by Professor Young, of Dartmouth College, Hanover,
N.H., has shown that the spectrum of the sierra some-

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[1] It is noteworthy how slowly the simple considerations involved in
the spectroscopic method of studying the prominences were developed,
and how difficult some astronomers found it to grasp the principles of
the method. At the meeting of the Astronomical Society where the
results obtained by Lockyer and Janssen were first announced, the
Astronomer-Royal spoke of the new method as if it were a sort of scien-
tific conjuring trick; yet its principles had been not only explained in
full by Huggins a year earlier, but had been already applied by Stone
at Greenwich.

times contains hundreds of bright lines, indicating the presence of the glowing vapour of iron, magnesium, sodium, lithium, titanium, and other elements. Only hydrogen, however, and the unknown element just mentioned appear to be constantly present in this solar envelope.

The actual study of the changes taking place in the solar prominences led to the discovery that very violent action must be taking place beneath the seemingly calm and silent surface of the glowing photosphere. In the essay, ' The Sun a Bubble,' which precedes the present, the chief features of the sun's condition in this respect have been dealt with at considerable length. As I am here dealing rather with eclipse discoveries than with the complete series of researches in solar physics effected since 1860, I need not now consider these signs of the intense activity of the great centre of the solar system. Let it suffice to state that the whole of that ruddy envelope which surrounds the photosphere to a height everywhere of at least eight thousand miles (so that a globe like our earth rolled over the sun's surface would bear the same proportion to the sierra that a cricket-ball bears to the grassy cover of an unmown field) is rent by repeated uprushes from within, which carry glowing gaseous matter to enormous distances above the outer visible limits of the sierra. And from time to time there are still more tremendous explosions and outbursts, seeming competent to carry matter from within the very bowels

of the sun to distances exceeding the span of the whole solar system.

Now that the prominences had been thus interpreted, it was natural that astronomers should renew their inquiries into the nature of the corona seen during total eclipses. There was first the question whether the corona is to be called the solar corona—that is, whether it really is a solar appendage—and then, if this question should be answered in the affirmative, there were further questions to be answered as to its constitution, structure, and condition.

The history of what happened at this stage is worth examining because of the illustration it affords of the usefulness of that careful investigation of known facts which is sometimes called theorizing, at other times speculation, but is not properly described by either term.[1]

Kirchhoff had expressed his belief that the corona is a solar atmosphere, and that to its action we are to

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[1] I may illustrate the distinction which is to be drawn between theorizing and the deduction of a theory from the investigation of evidence, by instances such as those questions which are set in school-books to lead to algebraical equations. If we try to guess the answer to a question of this kind, we may be said to be theorizing—we try one theory after another, and whether we light or not upon the true reply, we are not following any regular or systematic process. But if we solve such a question by the proper algebraical process we are in reality analysing the available evidence systematically. Each step brings us nearer and nearer to the result we require; and the process either leads us to that result or else shows us to what extent the evidence is insufficient, as in problems of the class called indeterminate. Of course the processes thus applied to the conditions of the original question can only educe what is already really present in the terms of the question; but no one on that account questions the usefulness of such processes.

attribute the presence of the dark lines in the solar spectrum. He also mentioned, as affording testimony in favour of this view, the fact that the sun's disc is less brilliant near the edge than in the middle, 'as though the globe of the sun were surrounded by a deep atmosphere.' I myself, attracted like many others to the interesting questions which five or six years ago were rife in the scientific world, was struck by the fact that when the last-named piece of evidence is closely examined it must be interpreted quite otherwise than Kirchhoff and others had supposed. The manifest darkening round the edge of the sun's disc, if to be explained by a solar atmosphere, implies a *relatively very shallow* envelope, not a deep envelope. If we look at a small opaque globe enclosed in the middle of a large glass globe, the line of sight passes through nearly the same range of glass, whether we look at the edge or at the middle of the small globe. But if we look at a large opaque globe coated with a uniform thin film of glass, the line of sight passes through a much greater range of glass when we look at the edge of the opaque globe than when we look towards its centre. Since then the range of the absorbing atmosphere is manifestly much greater near the edge than near the middle of the sun's disc, the inference seemed to me absolutely certain that the sun has a relatively shallow envelope—shallower far than the sierra; and to this envelope, not to the corona, it seemed to me that we must ascribe the multitudinous dark lines of the solar spectrum. In other words, I regarded it as certain

that a solar atmosphere (too shallow to be detected by any ordinary means) exists, inside the sierra, but outside the photosphere, and that this atmosphere is composed of the vapours of all the elements corresponding to the solar dark lines. But while a simple but demonstrative line of reasoning thus led to the rejection of one special piece of evidence which Kirchhoff had adduced in support of the theory that the corona belongs to the sun, other evidence was available which proved this to be the case. It was not so much the positive evidence in favour of the solar theory of the corona, as the negative evidence by which all other available theories were disposed of, which in reality established the solar theory of the corona. It could be proved that if the corona was a phenomenon of our own atmosphere, its light ought to grow fainter towards the place of the eclipsed sun, whereas the light grows brighter. It could be proved that no lunar atmosphere exists which can account for the corona; while if the coronal beams were caused by the illumination of matter occupying the space between the earth and moon, then rapid changes of a striking nature would take place which had never been described in the records of any single eclipse. No other theory being possible, the conclusion was certain that the corona is a solar appendage.

But such reasoning is caviare to the general. Complete, positive, and (above all) easily understood evidence was required before such conclusions could be accepted. Fortunately such evidence was soon forth-

coming. In the total eclipse of 1869, the shadow of
the moon passed right athwart the United States; and
the astronomers and amateurs of America, with the
zeal for science which has long honourably distin-
guished them, set themselves to observe the pheno-
mena of the prominences, corona, &c., at so many
stations that the whole track of totality might almost
be said to have been one continuous observatory. The
corona was photographed, though not in a manner
which decided its position as a solar appendage. But
spectroscopic analysis disposed of the question quite
satisfactorily, by showing that the spectrum of the
corona contains certainly one bright line (some thought
there were three bright lines)—in other words, that a
portion of the corona's light comes from glowing gas.
Doubts were thrown upon this result, partly perhaps
because (with that noble insular arrogance which
foreigners admire so much) some of us on this side
of the Atlantic were disposed to regard American
science as in its childhood. We have had our eyes
opened since, and know that Americans, in all depart-
ments of science, can hold their own, if not more, with
the best men of science in Europe.[1]

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[1] Even lately, however, the great success of the Americans in ana-
lysing the light of the corona during the eclipse of 1869 has been
slurred over thus in an article by Mr. Lockyer :—' In this eclipse the
halo of light outside the prominence envelope was the subject of special
inquiry, and now this was photographed. At the same time that this
was done, it was established that there was some other substance lying
even outside the hydrogen.'—('Times,' Jan. 11, 1875.) It is very de-
sirable that European writers should do justice to their American fel-
low-workers, for otherwise there cannot be cordial union in scientific

During the total eclipse of December 1870, the doubts thus raised were to be finally disposed of by the superior skill of European, and especially of British, spectroscopists. But the Americans, with singular perversity, determined to take their share in the work. Nay, at one time it even seemed as though either they alone would observe the eclipse, or our astronomers would have to be content to go as passengers in an American ship, although the eclipse was to be observed close by us in Spain and Sicily. However, the Government was roused by this news ; a letter from the Astronomer-Royal, which had been a month or two unanswered, was found in some pigeon-hole, and Ministers were pleased graciously to accede to the request therein made. Three English parties were sent to observe the eclipse in Spain, Algeria, and Sicily, besides a private party, under Lord Lindsay, in Spain ; and the Americans divided their forces into two chief *corps-d'armée*, one operating in Spain, the other in Sicily.

So far as spectroscopic observation was concerned, little of the good fortune of the scientific campaign fell to the lot of the English observers. Huggins and his party in Algeria had the satisfaction of noting the phenomena of a rainy day in that region ; Lockyer and his party in Sicily were not more fortunate with

work. It has been with some pain that I have noticed, also, in a recently published work on the moon, very inadequate recognition of American work in photographing that luminary (earlier and more perfectly than in Europe).

the spectroscope. Professor Young, of America, however, reobserved the coronal bright line. The Italian astronomers, Secchi and Denza, saw two lines, one in the green part of the spectrum, the other in the yellow-green. The great success, however, on this occasion, was that of the photographers. Brothers (of Manchester) in Sicily, and Professor Winlock (Cambridge, U.S.) in Spain, secured photographs of the corona agreeing so perfectly in details as to show that the objects pictured were true solar appendages. (Brothers's picture is as yet unmatched so far as the extent of corona shown in it is concerned.)

But on this occasion a yet more remarkable discovery was effected by Young. He determined to test the question whether there is a shallow but exceedingly rich and complex envelope immediately above that glowing surface which we call the sun (though in reality we begin to perceive more and more clearly that the sun we see is only one particular portion of the ruling centre of the solar system). It was manifest to Young that by treating a total eclipse as, so to speak, an extension of ordinary instrumental means for analysing the sun's light, he might recognise the existence of an envelope too shallow to be dealt with at other times. The moon would act like a dark cover, gradually hiding more and more of the sun, until, for a few moments, the whole of the photosphere, but not the shallow envelope, would be concealed. (The case may be illustrated by slowly passing a penny over a florin, or a halfpenny over a shilling, and noting

how for a moment or two the raised edge alone of the
silver coin is seen.)   For the few seconds during which
the sun was thus concealed, the shallow envelope, if
such existed, remaining still visible, light would be
received from the latter alone, and whatever the nature
of this light, or, in other words, whatever the character
of the envelope, the spectroscope would show.   It
happened as Professor Young had expected.   The rain-
bow-tinted streak crossed by dark lines, which consti-
tutes the solar spectrum, disappeared the moment the
true photosphere was completely concealed, and there
then sprang suddenly into view a spectrum of bright
lines only !   Where the multitudinous dark lines of
the solar spectrum had been, were now seen multitu-
dinous bright lines of all the colours of the rainbow,
each dark line on any point of the rainbow-tinted solar
spectrum being replaced by a bright line of the colour
of that part of the spectrum.   So that it was clear that
the envelope thus discovered is formed of the same
gases which produce the dark lines of the solar spec-
trum ; or rather it was clear that the dark lines are
formed by the absorptive action of this envelope,
though the gases present in it are really glowing with
intense brilliancy.   It is only by comparison with the
still more intense light of the solar photosphere that
the lines corresponding to these gases appear dark.

Thus two new solar envelopes were recognised, or
at least their existence demonstrated, on this occasion
—one, the outer corona, lying high above the inner
corona and prominence-envelope, while the other lies

below the prominence-envelope, and even far within the sierra of which the prominence-envelope must be regarded as the outer portion.

Observe, then, how complex the sun already appeared, compared with the glowing orb in which astronomers formerly believed. The analysis of sun-spots had shown that at least three envelopes exist within the photosphere, or that three lower levels are revealed in the larger spots—viz. the level corresponding to the penumbral fringe, then that belonging to the dark umbra, and thirdly that belonging to the so-called black nucleus.[1] The photosphere itself marks the position of a fourth envelope, or at least of a fourth solar level. Fifth comes the shallow complex atmosphere discovered by Young. Sixth, the sierra discovered by Grant, Leverrier, and Secchi. Seventh, the prominence region. Eighth, the inner and brighter corona. And ninth, the outer radiated corona. As to the depth of these successive envelopes, it is probable that the lowest level of the deeper spots lies about 10,000 miles below the photosphere. Young's atmosphere extends some three or four hundred miles above the photosphere; the sierra from eight to ten thousand miles; the prominence region to a height of thirty or forty thousand miles, with occasional extensions to a hundred thousand miles or more; the brighter corona

[1] Professor Langley, of the Alleghany Observatory in America, by careful telescopic research, has shown that the real structure of the sun is far more complex than had been supposed. A picture of a typical portion of the sun's surface, recently published by him, surpasses in completeness anything yet achieved by telescopists.

to from two to three hundred thousand miles, with ex-
pansions in places to four or five thousand miles; while,
lastly, the outer corona is so jagged in outline that it
is difficult to define its extension, but certainly some of
its radiations reach to a distance of fully a million miles
from the glowing surface of the sun we see.   When we
note that some of the envelopes here spoken of as single
are in reality multiple—the shallow atmosphere in-
cluding probably some thirty or forty distinct envelopes,
the sierra nine or ten, the prominence region two or
three, and the two coronas perhaps nine or ten others
—it will be seen what an amazingly complex subject
of research the sun has become in modern times.   That
great discovery of Kirchhoff's, the interpretation of the
spectrum, which promised to make all clear to us, has
in reality only taught us to know more certainly what
inscrutable mysteries surround the glowing centre of
the planetary system.

    But the next eclipse after that of 1870—the Indian
eclipse of December 1871 — revealed fresh wonders,
showing that even the outer corona is but the inner
part of a solar envelope (or rather appendage) whose
outermost limit lies altogether beyond our ken.

    For on that occasion, besides the notable success
obtained by photographers, it was demonstrated that
the corona shines in part by reflecting the sun's light.
Janssen, the skilful French spectroscopist, succeeded in
recognising in the faint rainbow-tinted ribbon of light
(on which the bright coronal lines are seen as on a
background) dark lines corresponding to those which

are most conspicuous in the solar spectrum. Here, then, was the most convincing evidence of the existence of matter capable of effectively reflecting the sun's light. And no reasonable doubt can exist that the matter whose presence was thus indicated is no other than the meteoric and cometic matter which other researches had taught us to recognise as plentifully strewn throughout the regions around the sun. How far this matter extends we do not certainly know. The zodiacal light, which is now commonly explained as due to the light from millions of minute bodies, extends visibly at least as far as the orbit of the earth. The occurrence of meteoric displays caused by the passage of such bodies through our own air proves in another way the same fact. But we know also that some of the meteor systems through which our earth passes travel far beyond the orbits of Uranus and Neptune, even to distances more than double that of the outermost known planet. So that to those enormous distances, though with an almost infinite sparseness of distribution, the meteoric and cometic matter which is now associated with the coronal envelopes of the sun must be regarded as unquestionably extending.

Seeing, then, that the sun is found to be the centre of a system of envelopes so wonderful, rising higher and higher above his glowing surface until they merge into systems extending beyond the outermost known planet, it gives a new interest to eclipse observation to consider that, during the total obscuration of the bright central region which we call the sun, the outer parts of that

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amazingly complex orb become discernible. By day
the sun's light blinds us to hosts of orbs like himself,
which at night come into view. But by day also the
glory of the sun hides from us the wonderful system of
envelopes and appendages of which he is the centre,
and the lustre of day passes away so gradually after
sunset that the faint light of the solar envelopes does
not become discernible while the sun-surrounding
region is above the horizon. It is only when the nearly
hiding orb of the moon conceals the glowing central
orb, while all around remains within the range of
vision, that we perceive the envelopes and appendages
which are in reality the outer parts of the sun himself.
Then only can we study with advantage the fainter of
these envelopes, whether by direct telescopic scrutiny,
or by spectroscopic analysis, or by securing photographic
records.

It will therefore, I think, interest my readers to learn
what are the plans by which astronomers hope next
April (see date of essay) to extend their knowledge of
the sun's surroundings. As I write there are unfor-
tunately divided counsels in the astronomical camp ;
but I hope that when these lines appear the actual
plan of operations will not only have been settled to
the satisfaction of all, but that it will include both the
lines of research which I am now about to indicate.

In the first place it has been suggested that ad-
vantage ought to be taken of the present opportunity
to determine whether the envelopes surrounding the
sun, sympathise, so to speak, with the disturbances
affecting the central orb. We know that the sun-spots

wax and wane in number, attaining their successive
maxima at intervals of about eleven years, while in the
mid interval (or nearly so, for the wave of disturbance
is not quite symmetrical) not only are no spots seen,
but the whole surface of the sun presents an appearance
of uniformity singularly different from its ordinary
mottled aspect. Now the last four occasions on which
these minima of spot disturbance—or we may say these
indications of quiescence—took place, were the years
1833, 1843, 1855-56, and 1866-67. If these intervals
were exactly equal, we could confidently assign the next
epoch of probable quiescence; but it will be observed
that they are not equal, being successively ten years,
twelve-and-a-half years, and eleven years. The average
interval for these three periods somewhat exceeds eleven
years, and if the current period should have that
length, the next epoch of quiescence would occur in
1877-78. But if the current period should be no
longer than that between the minima of 1833 and
1843, the next minimum would occur in 1876-77. We
are now near enough to the probable epoch to make it
desirable to secure on this occasion such pictures of the
corona as would serve for comparison with those
obtained in 1870 and 1871, when the sun-spots were
almost at their maximum of frequency and size. The
next great total eclipse will be that of 1878, visible
under favourable conditions in America, and it is quite
possible that on that occasion the minimum of sun-
spot frequency will be more nearly approximated to.
Still it would be a pity to lose the present opportunity,

when also the totality will last considerably longer than in 1878.

Now no satisfactory or trustworthy pictures of the corona can be obtained except by photography. Nothing ever obtained by mere draughtsmanship has had the slightest real value. We know from the experience of past eclipses that the corona can be photographed, notwithstanding the delicacy of its light. Those, therefore, who wish to learn whether the corona sympathises with the sun in those perturbations to which the spots are due, have insisted on the desirability of obtaining good photographs of the corona on this occasion. And in this view I altogether agree with them.

On the other hand, a method of research of extreme delicacy and difficulty, but also promising results of extreme interest if successfully applied, has been proposed by certain students of solar physics. It has been found, by a method of research invented by Mitscherlich, and recently used by Mr. Lockyer, that the spectra of different elements show a greater or smaller number of lines, according to the varying conditions under which the glowing vapour of the element exists. And as the conditions of heat and pressure throughout the sun's whole mass necessarily vary with distance from the centre, it follows that particular lines may be indicated for lower levels, which are wanting at greater distances from the sun's centre. I am now speaking of matter outside those parts of the sun which are, as it were, concealed from view by the intense brightness of

the photospheric region; though of course there is
every reason to believe that within this region a similar
variety of structure exists, the most complex solar
regions (those which alone contain all the known
elements) being nearest to the centre.[1]

Now, if by any means the observers of the coming
eclipse could determine how high the envelopes showing
various spectral lines extend from the surface of the
sun, the result would clearly be one of great interest.
For not only would it show to what distance the
vapours of particular elements extend, but it would in-
dicate also the conditions of temperature and pressure
under which those vapours exist. But there is no
time during totality to deal with all these different
spectral lines, even at any given part of the sun's edge,
far less all round the sun. Fortunately the lines need
not be measured, however, in this slow way. Professor
Young pointed out nearly four years ago that, by re-

[1] 'From the absence of the characteristic lines of some metals, such
as gold, silver, platinum, &c., from the solar spectrum,' says Guillemin
in his 'Les Phénomènes de la Physique,' 'it was believed, at first, that
these bodies are not found in the sun, at least in the outer strata which
form its atmosphere ; but this conclusion is too absolute, as is shown
by new researches due to Mitscherlich [according to whom the presence
of certain substances in a flame has the effect of preventing the spectra
of other substances from being formed, of extinguishing their principal
lines, &c.'] We follow the translation edited by Mr. Lockyer, except
in the passage within the brackets, which is taken from the original—
having somehow disappeared in the translated edition, where it is re-
placed by the remark that probably certain observations by 'Frankland
and Lockyer before alluded to' (in the English edition) may explain
the researches of Mitscherlich. Unfortunately nothing in the English
version indicates either the nature of Mitscherlich's researches, or that
the French text has been departed from in this place !

verting to the original form of the spectroscope, each
envelope might be seen apart from the rest. When we
look at the sun through an ordinary prism (like one of
the glass drops of a chandelier), we see a spectrum
which in reality consists of a multitude of images of the
sun, of all colours of the rainbow, overlapping each
other so as to produce a ribbon of rainbow-tinted light.
If the sun only gave out a certain order of rèd light,
another of yellow, another of green, and so on, we
should see so many pictures of the sun, each well
defined, pictures of the intermediate tints being wanting.
The slit of a spectroscope is merely a device to make
the source of light as narrow as possible, so that the
images may overlap less, and that, if any are wanting,
dark spaces may appear. Now in the case of the solar
prominence-ring and corona during totality this device
is not wanted. The prominence-ring shines with four
special tints—red, orange-yellow, blue-green, and indigo.
If we look at the ring through a series of prisms, without
any slit, we shall see the single ring of prominences trans-
muted by the action of the prisms into four images—a
red ring of prominences, an orange-yellow ring, a blue-
green ring, and an indigo ring. Similarly with that
green part of the coronal light which in the ordinary
spectroscopic method produces the green 'line : when
the simple train of prisms is used this portion will pro-
duce a green image of the corona, or *of so much of the
corona as contains the glowing gas which gives this
green light.*

All this has been practically tested. During the

eclipse of December 1871, Respighi saw the several pictures of the prominence-ring and the green picture of the inner corona. *But,* the various images were not bounded on the outside by a well-defined edge. The light simply became too faint at the outside of these several ring pictures to be discerned, so that he could not tell how far the corresponding envelopes really extended. And in the case of the green image of the corona the visible extension was far less than the already proved extension of the gaseous matter which produces the green coronal light. Now, if it had been proposed on the occasion of the approaching eclipse to attempt to renew Respighi's experiment under more favourable conditions, all astronomers would probably have agreed that interesting results might be obtained, though they would have recognized also the fact that no observer, however skilful, could successfully observe, measure, and record the extension of the several solar envelopes. But a much more difficult task has been suggested—namely, to photograph simultaneously these several images, or as many of them as may possess sufficient photographic power to delineate themselves. We need not concern ourselves here to examine how the mechanical difficulties of the problem were to be overcome. Suffice it to say that by keeping the telescope fixed and following the solar movement with a perfect plane mirror, so driven by clockwork as to reflect the solar rays continuously into the telescope, the unwieldiness of the spectroscopic and photographic combination attached to the

telescope becomes of no detriment, since the heavily
burdened instrument is not required to follow the shift-
ing sun. But where astronomers are divided, or rather,
we may say, where astronomers really are at issue with
physicists, is on the subject of the possibility of getting
any photographs at all with light demonstratively so
feeble as the green light of the corona. And, oddly
enough, astronomers maintain that physicists are wrong
on the physical part of the question. The light of the
corona as a whole has been analysed, and it is as certain
as well can be that the green light is but a very small
portion of the total coronal light. The whole light
acting at once to form a photograph does not show the
full extension of the corona, the outskirts simply losing
themselves through excessive faintness; how then,
argue astronomers, can physicists expect that a minute
portion of that light can produce any photographic
trace? How much less can this minute portion be
expected to show the whole extension of the green solar
envelope!

Unfortunately the scheme thus proposed by physi-
cists excluded the photographing of the corona by the
method formerly used, or in any other satisfactory
manner. Yet, even if the hopes of the physicists were
well based, one great result of their success would have
consisted in the means afforded for comparing the ex-
tension of the gaseous green corona with that of the
corona shining by reflecting the sun's light. This com-
parison would have been even more interesting than any
which could be instituted between the various gaseous

envelopes.   However, as I write, an effort is being
made to secure the provision of adequate appliances for
obtaining good photographs of the corona by the old
method ; and, whether the new method is likely to fail
or not, no one is disposed to be very earnest in oppos-
ing it so long as it does not exclude the safer method.
Probably, when these lines appear, it will be known
that both methods are to be used, and the explanation
given above will enable the reader to understand what
is expected from either, and thus to appreciate the im-
portance of the news telegraphed home to us on the
6th of April.[1]

(From the *Cornhill Magazine* for March 1875).

THE WEATHER AND THE SUN.

THERE are few scientific questions of greater interest
than the inquiry whether it is possible to find a means
of predicting the weather for a long time in advance.
In former ages many attempts were made to solve this
problem by a reference to the motions of the heavenly
bodies.   Other methods of prediction were, indeed, in
vogue; but I am not here considering ordinary weather
portents, or mere scientific schemes for anticipating the
weather of two or three coming days: and, with a few
trifling exceptions, depending on observations of plants

[1] As anticipated in the above passage, the suggested method failed
utterly.

and animals, it is the case that the only wide rules for
predicting weather were based on the motions of the
sun and moon, the planets and the stars.   It must be
remembered that even astronomers of repute placed
faith, until quite recent years, in the seemingly absurd
tenets of judicial astrology.   We cannot greatly
wonder, therefore, if the more reasonable thesis that
the heavenly bodies determine weather changes, was
regarded with favour.   Accordingly we find Horrocks,
more than two centuries ago, drawing the distinction
here indicated, where he says that in anticipating
' storm and tempest' from a conjunction of Mercury
with the Sun, he coincides ' with the opinion of the
astrologers, but in other respects despises their more
puerile vanities.'   We find Bacon in like manner re-
marking that ' all the planets have their summer and
winter, wherein they dart their rays stronger or weaker,
according to their perpendicular or oblique direction.'
He says, however, that ' the commixtures of the rays of
the fixed stars with one another are of use in contem-
plating the fabric of the world and the nature of the
subjacent regions, but in no respect for predictions.'
Bacon remarks again that reasonable astrology (*Astro-
logia sana*) ' should take into account the apogees and
perigees of the planets, with a proper inquiry into what
the vigour of planets may perform of itself; for a
planet is more brisk in its apogee, but more communi-
cative in its perigee : it should include, also, all the
other accidents of the planets' motions, their accelera-
tions, retardations, courses, stations, retrogradations,
distances from the sun, increase and diminution of light,

eclipses, &c. : for all these things affect the rays of the planets, and cause them to act either weaker or stronger, or in a different manner.'

It is a remarkable circumstance that systems of weather prediction based on such considerations were not quickly exploded owing to their failure when tested by experience. Yet singularly enough it has scarcely ever happened that any wide system of interpretation has been devised, which has not been regarded with favour by its inventor long after it had been in reality disproved by repeated instances of failure. This remark applies to recent systems as well as to those invented in earlier times. Within the last twenty years, for example, methods of prediction based on the moon's movements, on the conjunctions of the planets, and on other relations, have been maintained with astonishing perseverance and constancy, in the face of what outsiders cannot but regard as a most discouraging want of agreement between the predicted weather and the actual progress of events. Here, as in so many cases of prediction, we find the justice of Bacon's aphorism, 'Men mark when they hit, and never mark when they miss.'

It is noteworthy, indeed, that the very circumstance which appears to present a fatal objection to all schemes of prediction based on the motions of the celestial bodies, supplies the means of imagining that predictions have been fulfilled. The objection I refer to is this,— we know that the weather is seldom alike over very wide regions, while nevertheless the celestial bodies present the same aspect towards the whole extent of such regions, or an aspect so nearly the same as to sug-

-gest that the same conditions of weather should prevail
if the weather really depended on the position of the
heavenly bodies.   It appears, then, that the inventor of
a really trustworthy system must have a distinct scheme
for each part of every continent,—nay, of every country,
if not of every county.   This objection is not taken
into account, however, by the inventors of systems,
while the fact on which it depends affords the means of
showing that each prediction has been fulfilled.   Thus,
suppose ' bad weather and much wind ' have been pre-
dicted on a certain day, and that day is particularly fine
and calm in London.   If this were urged as an objection
to the soundness of the system, the answer would run
somewhat on this wise—' Unquestionably it was fine in
London, but in North Scotland (or in France, or Spain, or
Italy, or at Jericho, as the case may be) there was very
gloomy weather, and in Ireland (suppose) quite strong
winds are reported to have prevailed in the afternoon.'
The readiness with which men satisfy themselves in
such cases, corresponds with that mischievous ingenuity
wherewith foolish persons satisfy themselves that a for-
tune-teller had foretold the truth, that a dream had
been fulfilled, a superstition justified, and so forth.

The tendency, at present, amongst those who are
desirous of forming a scheme of weather prediction, is
to seek the origin of our weather-changes in changes of
the sun's condition, and by determining the laws of the
solar changes to ascertain the laws which regulate
changes in the weather.

It may be remarked in passing, that this new phase

of the inquiry does not reject planetary influences alto-
gether.   The theory is entertained by many well-known
students of science that changes in the condition of the
sun are dependent on the varying positions of the
planets; so that if it should be established that our
weather-changes are connected with solar changes, we
should infer that indirectly the planets in their motions
rule the weather on our earth.

I propose now to consider the evidence relating to
the sun's influence, and to discuss the question (alto-
gether distinct, be it remarked) whether a means of
accurate weather prediction may be obtained *if* the
sun's influence be regarded as demonstrated.

There is one strong point in favour of the new
theory, in the fact that the sun is unquestionably the
prime cause of all weather-changes.   To quote the
words of Lieut.-Colonel Strange, an enthusiastic advo-
cate of the theory (and eager to have it tested at the
nation's expense), 'there can hardly be a doubt that
almost every natural phenomenon connected with cli-
mate can be distinctly traced to the sun as the great
dominating force, and it is a natural inference' (though
not, as he says, an unavoidable one) 'that the changes,.
and what we now call the uncertainties of climate are
connected with the constant fluctuations which we
know to be perpetually occurring in the sun itself.'   I
may proceed, indeed, in this place, to quote the follow-
ing words in which Colonel Strange enunciates the
theory itself which I am about to discuss, and its con-
sequences :—' The bearing of climatic changes on a vast.

array of problems connected with navigation, agriculture, and health, need but be mentioned to show the importance of seeking in the sun, where they doubtless reside, for the causes which govern these changes. It is indeed my conviction that of all the fields now open for scientific cultivation, there is not one which, quite apart from its transcendent philosophical interest, promises results of such high utilitarian value, as the exhaustive systematic study of the sun.'

It cannot be doubted, I think, that if anything like what is here promised could be hoped for from the study of the sun, it would be a matter of more than national importance to undertake the task indicated by Colonel Strange. The expense of new observatories for this special subject of study would in that case be very fully repaid. It would be worth while to employ the most skilful astronomers at salaries comparable with those which are paid to our Government ministers; it would be well to secure on corresponding terms the advice of those most competent to decide on the instrumental requirements of the case; and in fact the value of the work which is at present accomplished at Greenwich, great though that value is, would sink into utter insignificance, in my judgment, compared with the results flowing in the supposed case from the proposed 'exhaustive and systematic study' of the great central luminary of the planetary system.

The subject we are to discuss is manifestly therefore of the utmost importance, and cannot be too carefully dealt with. It would be a misfortune on the one hand

to be led by careless reasoning to underestimate the
chances in favour of the proposed scheme, while on the
other it would be most mischievous to entertain un-
founded expectations where the necessary experiments
must be of a costly nature, and where science would be
grievously discredited should it be proved that the
whole scheme was illusory.

We note, first, that besides being 'the great domi-
nating force' to which all natural phenomena connected
with climate are due, the sun has special influence on
all the most noteworthy *variations* of weather. The
seasons are due to solar influence; and here we have an
instance of a power of prediction derived from solar
study, though belonging to a date so remote that we
are apt to forget the fact. It seems so obvious that
summer will be on the whole warmer than winter, that
we overlook the circumstance that at some epoch or
other this fact, at least in its relation to the apparent
motions of the sun, must have been recognised as a
discovery. Men must at one time have learned, or per-
haps we should rather say, each race of men must at
one time have noticed, that the varying warmth on
which the processes of vegetation depend, corresponds
with the varying diurnal course of the sun. So soon as
this was noticed, and so soon as the periodic nature of
the sun's varying motions had been ascertained, men
had acquired in effect the power of predicting that at
particular times or seasons, the weather on the whole
would be warmer than at other seasons. In other
words, so soon as men had recognised the period we

call the *year*, they could predict that one half of each
year would be warmer than the other half. Simple as
this fact may seem, it is important to notice it as the
beginning of weather prediction; for as will presently
appear, it has an important bearing on the more com-
plex questions at present involved in the prognostication
of weather-changes.

It became manifest almost as soon as this discovery
had been made, that the weather of particular days or
even of weeks and longer periods could not, by its
means, be predicted. A week in summer may be cold,
and a week in winter may be warm; nor, so far as is
even yet known, is there a single part of any year
the temperature of which can be certainly depended
upon, at least within the temperate zone. In certain
tropical regions there are tolerably constant weather
variations; but so far is this from being the case in the
temperate zones of either hemisphere, that it is impos-
sible to affirm certainly, even that during a week or
fortnight at any given summer season there will be one
hot day, or that during a corresponding period in
winter there will be one day of cold weather.

It became manifest also, at an early epoch, that
terrestrial conditions must be intimately involved in all
questions of weather, since the year in different
countries in the same latitudes presents different
features. Such differences are of two kinds,—those
which have a tendency to be constant, and those which
are in their nature variable. For example, the annual
weather in Canadian regions having the same range of

latitude as Great Britain, differs always to a very
marked degree, though not always to the same degree,
from that which prevails in this country; here then we
have a case of a constant difference due unquestionably
to terrestrial relations. Again, when we have a hot or
dry summer in this country, warm or damp weather
may prevail in other countries in the same latitudes,
and *vice versâ*; differences of this kind are ordinarily[1]
variable, and in the present position of weather-science
are regarded as accidental.

Hitherto, weather-science has depended solely on
the study of these terrestrial effects as they vary under
varying conditions. Modern meteorological research is
confined to the record and study of the actual condition
of the weather from day to day at selected stations in
different countries. It cannot be denied that the in-
quiry has not been attended with success. At vast ex-
pense millions of records of heat, rainfall, winds, clouds,
barometric pressure, and so on have been secured; but

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[1] I use this qualifying word, because some differences of the kind
are more or less regular. Thus when there is a dry summer in certain
regions in the west of Europe, there is commonly a wet summer in
easterly regions in the same latitude, and *vice versâ*, the difference
simply depending on the height at which the clouds travel which are
brought by the south-westerly counter-trade winds. When these clouds
travel high, they do not give up their moisture until they have travelled
far inland or towards the east; when they travel low, their moisture is
condensed so soon as they reach the western landslopes. It is not un-
commonly the case, again, that when we in England have dry summers,
much rain falls on the Atlantic, and our drought is simply due to the
fall of this rain before the clouds from the south-west have reached us.
More commonly, however, drought in England is due to the delay of
the downfall, in consequence of the clouds from the south-west travelling
at a greater height than usual.

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hitherto no law has been recognized in the variations
thus recorded,—no law at least from which any con-
stant system of prediction for long periods in advance
can be deduced.

On this point I shall quote first a remarkable say-
ing of Sir W. Herschel's, which appears to me, like
many such sayings of his, to be only too applicable to
the present state of science. In endeavouring to in-
terpret the laws of weather, ' we are in the position,'
Herschel remarks, ' of a man who hears at intervals a
few fragments of a long history related in a prosy, un-
methodical manner. A host of circumstances omitted
or forgotten, and the want of connection between the
parts, prevents the hearer from obtaining possession of
the entire history. Were he allowed to interrupt the
narrator, and ask him to explain the apparent contra-
dictions, or to clear up doubts at obscure points, he
might hope to arrive at a general view. The questions
that he would address to nature, are the very experi-
ments of which we are deprived in the science of
meteorology.'

The late Professor De Morgan, indeed, selected
meteorology as the subject on which, above all others,
systematic observations had been most completely
wasted,—as a special instance of the failure of the true
Baconian method (which be it noticed is not, as is so
commonly supposed, the modern scientific method).
' There is an attempt at induction going on,' says De
Morgan, ' which has yielded little or no fruit—the ob-
servations made in the meteorological observatories.
This attempt is carried on in a manner which would

have caused Bacon to dance for joy' (query); 'for he
lived in times when Chancellors did dance. Russia,
says M. Biot, is covered by an army of meteorographs,
with generals, high officers, subalterns, and privates,
with fixed and defined duties of observation. Other
countries, also, have their systematic observatories.
And what has come of it? Nothing, says M. Biot, and
nothing will ever come of it: the veteran mathemati-
cian and experimental philosopher declares, as does Mr.
Ellis' (Bacon's biographer), 'that no single branch of
science has ever been fruitfully explored in this way.'
A special interest attaches, I may remark, to the
opinion of M. Biot, because it was given upon the pro-
posal of the French Government to construct meteoro-
logical observatories in Algeria.

It is well known that our Astronomer Royal holds a
similar opinion. De Morgan thus quaintly indicates
his interpretation of one particular expression of Sir
G. Airy's opinion :—' In the report to the Greenwich
Board of Visitors, for 1867, the Astronomer Royal,
speaking of the increase of meteorological observa-
tories, remarks, " Whether the effect of this movement
will be that millions of useless observations will be
added to the millions that already exist, or whether
something may be expected to result which will lead to
a meteorological theory, I cannot hazard a conjecture."
This *is* a conjecture, and a very obvious one; if Mr.
Airy would have given $2\frac{3}{4}d$. for the chance of a meteoro-
logical theory formed by masses of observations, he
would never have said what I have quoted.'

The simple combination of terrestrial considerations with the effects due to the sun's varying daily path having thus far failed to afford any interpretation of the varying weather from year to year, it is natural to inquire whether the variations in the sun's condition from year to year may not supply the required means of interpreting and hence of predicting weather-changes. We know that the sun's condition does vary, because we sometimes see many large spots upon his surface, whereas at others he has no spots, or few and small ones. We can scarcely doubt that these variations affect the supply of heat and light, as well as of chemical action and possibly of other forms of force ; and hence we are certainly dealing with a *vera causa,* though whether this real cause be an efficient cause of weather-changes remains yet to be determined.

It may perhaps be well to inquire, however, in the first place, whether any peculiarities of weather can be traced to another circumstance which ought to be at least as efficient, one would suppose, as any changes in the sun's action due to the spots. I refer to his varying distance from the earth. It is known doubtless to all my readers that in June and July, although these are our summer months, the sun is farther away than in December,—and this, not by an inconsiderable distance, but by more than three millions of miles. Accordingly, on a summer day in our hemisphere we receive much less heat than is received on a summer day in the southern hemisphere. Or instead of comparing our summer heat with summer heat in

the southern hemisphere, we may make comparison between the quantity of heat received by the whole earth on a day in June and on a day in December. Either way of viewing the matter is instructive; and I believe many of my readers will be surprised when they hear what is the actual amount of difference.

We receive in fact, on June 30, less heat and light than dwellers at our antipodes receive on December 30, by the amount which would be lost if an opaque disc having a diameter equal to one-fourth of the sun's,[1] came upon the sun's face as seen on December 30 at our antipodes. It need hardly be said that no spots whose effects would be comparable with those produced by such a disc of blackness have ever been seen upon the face of the sun. Spots are not black or nearly black, even in their very nucleus. The largest ever seen has not had an extent approaching that of our imagined black disc, even when the whole dimensions of the spot,—nucleus, umbra, and penumbra, have been taken into account. Moreover, all round a spot there is always a region of increased brightness, making up to a great degree, if not altogether, for the darkness of the spot itself. So that unquestionably the summer heat in the southern hemisphere exceeds the

[1] It is easily shown that such would be the size of the imagined black disc. For the sun's distance varies from about 93,000,000 of miles to about 90,000,000, or in the proportion of 31 to 30. Hence the size of his disc varies in the proportion of 31 times 31 to 30 times 30, or as 961 to 900. The defect of the latter number 900 amounts to 61, which is about a sixteenth part of the larger number. But a black disc having a diameter equal to a quarter of the sun's would cut off precisely a sixteenth part of his light and heat, which was the fact to be proved.

summer heat in our hemisphere to a much more
marked degree than the heat given out by the sun
when he is without spots exceeds the heat of a spotted
sun.

It is, however, rather difficult to ascertain what
effect is to be ascribed to this peculiarity. It is certain
that the Australian summer differs in several important
respects from the European summer ; but it is not easy
to say how much of the difference is due to the pecu-
liarity we have been considering, and how much to the
characteristic distinction between the northern and
southern halves of the earth,—the great excess of
water surface over land surface in the southern hemi-
sphere. It is worthy of notice that, even in this case,
where we cannot doubt that a great difference must
exist in the solar action at particular seasons, we find
ourselves quite unable to recognise any peculiarities of
weather as *certainly* due to this difference.

I have spoken of a second way of viewing the differ-
ence in question, by considering it as it affects the
whole earth. The result is sufficiently surprising. It
has been shown by the researches of Sir J. Herschel
and Pouillet, that on the average our earth receives
each day a supply of heat competent to heat an ocean
260 yards deep over the whole surface of the earth
from the temperature of melting ice to the boiling
point. Now, on or about December 30, the supply is
one-thirtieth greater, while on or about June 30, the
supply is one-thirtieth less. Accordingly, on June 30,
the heat received in a single day would be competent

only to raise an ocean 251⅓ yards deep from the freezing
to the boiling point, whereas on December 30 the
heat received from the sun would so heat an ocean 268⅔
yards deep. The mere excess of heat, therefore, on
December 30 over that on June 30 would suffice to
raise an ocean more than 17 yards deep and covering
the whole earth, from the freezing point to the tempe-
rature of boiling water! It will not be regarded as
surprising if terrestrial effects of some importance
should follow from so noteworthy an excess, not merely
of light and heat, but of gravitating force, of magnetic
influence, and of actinic or chemical action, exerted
upon the earth as a whole. Accordingly we find that
there is a recognizable increase in the activity of the
earth's magnetism in December and January as com-
pared with June and July. But assuredly the effect
produced is not of such a character as to suggest that
we should find the means of predicting weather *if* it
were possible for us *now* to discover any solar law of
change resulting in a corresponding variation of solar
action upon the earth.

This leads us to consider the first great law of solar
change as distinguished from systematic variations like
the sun's varying change of distance and his varying
daily path on the heavens. This law is that which
regulates the increase and decrease of the solar spots
within a period of about eleven years. The sun's con-
dition does not, indeed, admit of being certainly pre-
dicted by this law, since it not unfrequently happens
that the sun shows few spots for several weeks together,

in the very height of the time of spot-frequency, while on the other hand it often happens that many and large spots are seen at other times. Nevertheless, this general law holds, that, on the whole, and taking one month with another, there is a variation in spot-frequency, having for its period an interval of rather more than eleven years.

Now, the difference between a year of maximum spot-frequency, and one of minimum frequency, is very noteworthy, notwithstanding the exceptional features just mentioned, which show themselves but for short periods. This will be manifest on the consideration of a few typical instances. Thus, in the year 1837, the sun was observed on 168 days, during which he was not once seen without spots, while no less than 333 new groups made their appearance. This was a year of maximum spot-frequency. In 1843, the sun was observed on 312 days, and on no less than 149 of these no spots could be seen, while only 34 new groups made their appearance. This was a year of minimum spot-frequency. Passing to the next maximum year, we find that in 1848 the sun was observed on 278 days, during which he was never seen without spots, while 330 new spots made their appearance. In 1855 and 1856 together, he was observed on 634 days, on 239 of which he was without spots, while only 62 new groups made their appearance. The next maximum was not so marked as usual, that is, there was not so definite a summit, if one may so speak, to the wave of increase; but the excess of spot-frequency was none the less de-

cided. Thus, in the four years, 1858, '59, '60, '61, the sun was observed on 335, 343, 333, and 322 days, *on not one of which he was spotless*, while the numbers of new groups for these four years were, respectively, 202, 205, 211, and 204. The minimum in 1867 was very marked, as 195 days out of 312 were without spots, and only 25 new groups appeared. The increase after 1867 was unusually rapid, since in 1869 there were no spotless days, and 224 new groups were seen, though the sun was only observed on 196 days. The number of spots in 1870, 1871, and 1872, as well as their magnitude and duration, have been above what is usual, even at the period of maximum spot-frequency.

From all this it will be manifest that we have a well-marked peculiarity to deal with, though not one of perfect uniformity. Next to the systematic changes already considered, this alternate waxing and waning of spot-frequency might be expected to be efficient in producing recognisable weather changes. Assuredly, if this should not appear to be the case, we should have to dismiss all idea that the sun-spots are weather-rulers.

Now, from the first discovery of spots, it was recognised that they must, in all probability, affect our weather to some degree. It was noticed, indeed, that our auroras seemed to be in some way influenced by the condition of the sun's surface, since they were observed to be more numerous when there are many spots than when there are few or none. Singularly enough, the

effect of the spots on temperature was not only inquired
into much later (for we owe to Cassini and Mairan the
observation relating to auroras), but was expected to be
of an opposite character from that which is in reality
produced. Sir W. Herschel formed the opinion that
when there are most spots the sun gives out most heat,
notwithstanding the diminution of light where the spots
are. He sought for evidence on this point in the price
of corn in England, and it actually appeared, though
by a mere coincidence, that corn had been cheapest in
years of spot-frequency, a result regarded by Herschel
as implying that the weather had been warmer on the
whole in those years.[1] It was well pointed out, however,
by Arago, that 'in these matters we must be careful
how we generalise facts before we have a very consider-
able number of observations at our disposal.' The
peculiarities of weather in a single and not extensive
country like England, are quite insufficient to supply

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[1] When Herschel made his researches into this subject, the law of
spot-frequency had not been discovered. He would probably have found
in this law, as some have since done, the explanation of the seven years
of plenty and the seven years of famine typified by the fat kine and
lean kine of Joseph's dream. For if there were a period of eleven
years in which corn and other produce of the ground waxed and waned
in productiveness, it would be not at all unlikely that whenever this
waxing and waning chanced to be unusually marked, there would
result two series of poor and rich years apparently ranging over four-
teen instead of eleven years. We have seen, above, that the waves of
spot-waxing and spot-waning are not all alike in shape and extent.
Whenever then a wave more marked than usual came, we should
expect to find it borrowing, so to speak, both in trough and crest, from
the waves on either side. It would require but a year or so either way
to make the wave range over fourteen years; and observed facts even
during the last half-century only, show this to be no unlikely event.

an answer to the wide question dealt with by Herschel. The weather statistics of many countries must be con-- sidered and compared. Moreover, very long periods of time must be dealt with.[1]

M. Gautier of Geneva and, later, MM. Arago and Barratt made a series of researches into the tabulated temperature at several stations, and for many successive years. . They arrived at the conclusion that, on the whole, the weather is coolest in years of spot-frequency.

But recently the matter has been more closely scrutinised, and it has been found that the effects due to the great solar-spot period, although recognisable, are by no means so obvious as had been anticipated.

These effects may be divided into three classes,— those affecting (1) temperature, (2) rainfall, (3) terres- trial magnetism.

As respects the first, it has been discovered that when *underground* temperatures are examined, so that local and temporary causes of change are eliminated, there is a recognisable diminution of temperature in years when spots are most frequent. We owe this dis- covery to Professor C. P. Smyth, Astronomer Royal for Scotland. The effect is very slight; indeed, barely recognisable. I have before me, as I write, Professor Smyth's chart of the quarterly temperatures from 1837 to 1869, at depths of 3, 6, 12, and 24 French feet. Of course, the most remarkable feature even at the depth of 24 feet, is the alternate rise and fall with the seasons. ˉ But it is seen that while the range of rise and fall remains very nearly constant, the crests and troughs.

of the waves lie at varying levels. After long and careful scrutiny, I find myself compelled to admit that I cannot find the slightest evidence in *this* chart of a connection between underground temperatures and the eleven-years period'of sun spots. I turn, therefore, to the chart in which the annual means are given; and noting in the means at the lesser depths ' confusion worse confounded' (this, of course, is no fault of Professor Smyth's, who here merely records what had actually taken place), I take the temperatures at a depth of 24 French feet. Now, neglecting minor features, I find the waves of temperature thus arranged. They go down to a little more than $46\frac{1}{2}$ degrees of the common thermometer in 1839-40; rise to about $47\frac{3}{4}$ degrees in 1847; sink to $47\frac{1}{4}$ degrees in 1849; mount nearly to $47\frac{3}{4}$ degrees again in 1852-53; are at 47 degrees in 1856-57; are nearly at 48 degrees in 1858-59; then they touch 47 degrees three times (with short periods of rising between), in 1860, 1864, and 1867; and rise above $47\frac{1}{2}$ degrees in 1869. If we remember that there were maxima of spots in 1837, 1848, 1859-60, and 1870, while there were minima in 1843 and 1855-56, I think it will be found to require a somewhat lively imagination to recognise a very striking association between the underground temperature and the sun's condition with respect to spots. If many spots imply diminution of heat, how does it come that the temperature rises to a maximum in 1859, and again in 1869? if the reverse, how is it that there is a minimum in 1860? I turn, lastly, to the chart in which the sun-

spot waves and the temperature waves are brought into actual comparison, and I find myself utterly unable to recognise the slightest association between them. Nevertheless, I would not urge this with the desire of in any way throwing doubt upon the opinion to which Professor Smyth has been led, knowing well that the long and careful examination he has given to this subject in all its details, may have afforded ample though not obvious evidence for the conclusions at which he has arrived. I note also, that, as he points out, Mr. Stone, director of the Cape Town Observatory, and Mr. Cleveland Abbe, director of the Cincinnati Observatory, have since, ' but it is believed quite independently, published similar deductions touching the earth's temperature in reference to sun-spots.' All I would remark is, that the effect is very slight and very far from being obvious at a first inspection.

Next as to rainfall and wind.

Here, again, we have results which can hardly be regarded as striking, except in the forcible evidence they convey of the insignificance of the effects which are to be imputed to the great eleven-year spot period. We owe to Mr. Baxendell, of Manchester, the most complete series of investigations into this subject. He finds that at Oxford, during the years when sun-spots were most numerous, the amount of rainfall under west and south-west winds was greater than the amount under south and south-east winds; while the reverse was the case in years when spots were few and small. Applying corresponding processes to the meteorological

records for St. Petersburg, he finds that a contrary state
of things prevailed there.   Next we have the evidence
of the Rev. R. Main, director of the Radcliffe Observa-
tory at Oxford, who finds that westerly winds are
slightly more common when sun-spots are numerous
than at other times.   And lastly, Mr. Meldrum of
Mauritius notes that years of spot-frequency are charac-
terized on the whole by a greater number of storms
and hurricanes, than years when the sun shows few
spots.

The association between the sun-spot period and
terrestrial magnetism is of a far more marked character,
though I must premise that the Astronomer Royal,
after careful analysis of the Greenwich magnetic re-
cords, denies the existence of any such association what-
ever.   There is, however, a balance of evidence in its
favour.   It seems very nearly demonstrated that the
daily sway of the magnetic needle is greatest when sun-
spots are numerous, that magnetic storms are somewhat
more numerous at such times, and that auroras are
more commonly seen.   Now it has been almost demon-
strated by M. Marié Davy, chief of the meteorological
division in the Paris Observatory, that the weather is
affected in a general way by magnetic disturbances. So
that we are confirmed in the opinion that indirectly, if
not directly, the weather is affected to some slight
degree by the great sun-spot period.

Still I must point out that not one of these cases of
agreement has anything like the evidence in its favour
which had been found for an association between the

varying distance of Jupiter and the sun-spot changes.
For eight consecutive maxima and minima this associa-
tion has been strongly marked, and might be viewed as
demonstrated,—only it chances unfortunately that for
two other cases the relation is *precisely reversed*; and
in point of fact, whereas the period now assigned to the
great sun-spot wave is eleven years and rather less than
one month, Jupiter's period of revolution is eleven
years and about *ten* months, a discrepancy of nine
months, which would mount up to five and a half
years (or modify perfect agreement into perfect dis-
agreement) in seven or eight cycles.

But accepting the association between weather and
the sun-spot changes as demonstrated (which is granting
a great deal to believers in solar weather-prediction),have
we any reason to believe that by a long-continued study
of the sun the great problem of foretelling the weather
can be solved ?   This question, as I have already
pointed out, must not be hastily answered.   It is one of
national, nay, of cosmopolitan importance.   If answered
in the affirmative, there is scarcely any expense which
would be too great for the work suggested; but all the
more careful must we be not to answer it in the affirma-
tive, if the true answer should be negative.

But it appears to me that so soon as the considera-
tions dealt with above have been fairly taken into
account, there can be no possible doubt or difficulty in
replying to the question.   The matter has in effect,
though not in intention, been tested experimentally,
and the experiments, when carried out under the most

favourable conditions, have altogether failed. To show
that this is so, I take the position of affairs before
Schwabe began that fine series of observations which
ended in the discovery of the great spot-period of
eleven years. Let us suppose that at that time the
question had been mooted whether it might not be
possible, by a careful study of the sun, to obtain some
means of predicting the weather. The argument would
then have run as follows:—'The sun is the great
source of light and heat; that orb is liable to changes
which must in all probability affect the supply of light
and heat; those changes may be periodical and so pre-
dictable; and as our weather must to some extent
depend on the supply of light and heat, we may thus
find a means of predicting weather changes.' The
inquiry might then have been undertaken, and un-
doubtedly the great spot-period would have been de-
tected, and with this discovery would have come that
partial power of predicting the sun's condition which
we now possess—that is, the power of saying that in
such and such a year, taken as a whole, spots will be
numerous or the reverse. Moreover, meteorological
observations conducted simultaneously would have
shown that, as the original argument supposed, the
quantity of heat supplied by the sun varies to a slight
degree with the varying condition of the sun. Corre-
sponding magnetic changes would be detected; and
also those partial indications of a connection between
phenomena of wind and rain and the sun's condition
which have been indicated above. All this would be

exceedingly interesting to men of science. *But*—supposing all this had been obtained at the nation's expense, and the promise had been held out that the means of predicting weather would be the reward, the non-scientific tax-paying community might not improbably inquire what was the worth of these discoveries to the nation or to the world at large. Be it understood that I am not here using the *cui bono* argument. As a student of science, I utterly repudiate the notion that before scientific researches are undertaken, it must be shown that they will *pay*. But it is one thing to adopt this mean and contemptible view of scientific research, and quite another to countenance projects which are based *ab initio* upon the ground that they will more than repay their cost.

If the nation made the inquiry above indicated, and under the circumstances mentioned, it would be very difficult, I think, to give a satisfactory reply. The tax-payers would say, ' We have supplied so many thousands of pounds to found national observatories for the cultivation of the physics of science, and we have paid so many thousands of pounds yearly to the various students of science who have kindly given their services in the management of these observatories ; let us hear what are the utilitarian results of all this outlay ? We do not want to hear of scientific discoveries, but of the promised means of predicting the weather.' The answer would be, ' We have found that storms in the tropics are rather more numerous in some years than others, the variations having a period of eleven

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years; we can assert pretty confidently that auroras follow a similar law of frequency; south-west winds blow more commonly at Oxford, but less commonly elsewhere, when the sun-spots, following the eleven-years period, are at a maximum; and more rain falls with south-westerly winds than with south-easterly winds at Oxford and elsewhere, but less at St. Petersburg and elsewhere, when sun-spots are most numerous, while the reverse holds when the spots are rare.' I incline to think that on being further informed that these results related to averages only, and gave no means of predicting the weather for any given day, week, or month, even as respects the unimportant points here indicated, the British tax-payer would infer that he had thrown away his money. I imagine that the army of observers who had gathered these notable results would be disbanded rather unceremoniously, and that for some considerable time science (as connected, at any rate, with promised ' utilitarian' results) would stink in the nostrils of the nation.

But this is very far, indeed, from being all. Nay, we may almost say that this is nothing. Astronomers *know* the great spot-period; they have even ascertained the existence of longer and shorter periods less marked in character; and they have ascertained the laws according to which other solar features besides the spots vary in their nature. It is certain that whatever remains to be discovered must be of a vastly less marked character. If then the discovery of the most striking law of solar change has led to no results having the

slightest value in connection with the problem of weather-prediction, if periodic solar changes of a less marked character have been detected which have no recognisable bearing on weather changes, what can be hoped from the recognition of solar changes still more recondite in their nature? It is incredible that the complex phenomena involved in meteorological relations regarded as a whole, those phenomena which are but just discernibly affected by the great sun-spot period, should respond to changes altogether insignificant even when compared with the development and decay of a single small sun-spot. It appears to me, therefore, that it is the duty of the true lover of science to indicate the futility of the promises which have been mistakenly held out; for it cannot be to the credit of science, or ultimately to its advantage, if Government assistance be obtained on false pretences for any branch of scientific research.

(From the *St. Paul's Magazine*.)

FINDING THE WAY AT SEA.

THE wreck of the *Atlantic*, followed closely by that of the *City of Washington* nearly on the same spot, has led many to inquire into the circumstances on which depends a captain's knowledge of the position of his ship. In each case, though not in the same way, the

ship was supposed to be far from land, when in reality
quite close to it. In each case, in fact, the ship had
oversailed her reckoning. A slight exaggeration of
what travellers so much desire—a rapid passage—
proved the destruction of the ship, and in one case
occasioned a fearful loss of life. And although such
events are fortunately infrequent in Atlantic voyages,
yet the bare possibility that besides ordinary sea risks
a ship may be exposed to danger from simply losing her
way, suggests unpleasant apprehensions as to the general
reliability of the methods in use for determining where
a ship is, and her progress from day to day.

I propose to give a brief sketch of the methods in
use for finding the way at sea, in order that the general
principles on which safety depends may be recognised by
the general reader.

It is known, of course, to everyone, that a ship's
course and rate of sailing are carefully noted through-
out her voyage. Every change of her course is taken
account of, as well as every change in her rate of ad-
vance, whether under sail or steam or both combined.
If all this could be quite accurately managed, the posi-
tion of the ship at any hour could be known, because it
would be easy to mark down on a chart the successive
stages of her journey, from the moment when she left
port. But a variety of circumstances render this im-
possible.

To begin with, the *exact* course of a ship cannot be
known, because there is only the ship's compass to de-
termine her course by, and a ship's compass is not an

instrument affording perfectly exact indications. Let
anyone on a sea voyage observe the compass for a
short time, being careful not to break the good old
rule which forbids speech to the 'man at the wheel,'
and he will presently become aware of the fact that
the ship is not kept rigidly to one course even for a
short time. The steersman keeps her as near as he
can to a particular course, but she is continually de-
viating, now a little on one side now a little on the
other of the intended direction; and even the general
accuracy with which that course is followed is a matter
of estimation, and depends on the skill of the indi-
vidual steersman. Looking at the compass card, in
steady weather, a course may seem very closely fol-
lowed; perhaps the needle's end may not be a hun-
dredth part of an inch (on the average) from the
position it should have. But a hundredth part of an
inch on the circumference of the compass card, would
correspond to a considerable deviation in the course of
a run of twenty or thirty knots; and there is nothing
to prevent the errors so arising from accumulating in
a long journey until a ship might be thirty or forty
miles from her estimated place. To this may be added
the circumstance that the direction of the needle is
different in different parts of the earth. In some
places it points to the east of the north, in others to
the west. And although the actual 'variation of the
compass,' as this peculiarity is called, is known in a
general way for all parts of the earth, yet such know-
ledge has no claim to actual exactness. There is, also,

an important danger, as recent instances have shown, in the possible change of the position of the ship's compass on account of iron in her cargo.

But a far more important cause of error, in deter-- minations merely depending on the log-book, is that arising from uncertainty as to the ship's rate of pro- gress. The log-line gives only a rough idea of the ship's rate at the time when the log is cast;[1] and of course a ship's rate does not remain constant, even when she is under steam alone. Then again, currents carry the ship along sometimes with considerable ra- pidity; and the log-line affords no indication of their action: while no reliance can be placed on the esti- mated rates even of known currents. Thus the dis- tance made on any course may differ considerably from the estimated distance; and when several days' sailing are dealt with an error of large amount may readily accumulate.

For these and other reasons, a ship's captain places little reliance on what is called ' the day's work,' that is, the change in the ship's position from noon to noon as estimated from the compass courses entered in the

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[1] The log is a flat piece of wood of quadrantal shape, so loaded at the rim as to float with the point (that is, the centre of the quadrant) uppermost. To this a line about 300 yards long is fastened. The log is thrown overboard and comes almost immediately to rest on the sur- face of the sea, the line being suffered to run freely out. Marks on the log-line divide it into equal spaces, called *knots*, of known length, and by observing how many of these run out, while the sand in a half- minute hour-glass is running, the ship's rate of motion is inferred. The whole process is necessarily rough, since the line cannot even be tautened.

log-book, and the distances supposed to be run on
these courses. It is absolutely essential that such esti-
mates should be carefully made, because under un-
favourable conditions of weather there may be no
other means of guessing at the ship's position. But
the only really reliable way of determining a ship's
place is by astronomical observations. It is on this
account that the almanac published by the Admiralty,
in which the position and apparent motions of the
celestial bodies are indicated four or five years in ad-
vance, is called, *par excellence,* the *Nautical* Almanac.
The astronomer in his fixed observatory finds this al-
manac essential to the prosecution of his observations ;
the student of theoretical astronomy has continual
occasion to refer to it: but to the sea-captain the
Nautical Almanac has a far more important use. The
lives of sailors and passengers are dependent upon its
accuracy. It is, again, chiefly for the sailor that our
great nautical observatories have been erected and that
our Astronomer-Royal and his officers are engaged.
What other work they may do is subsidiary, and as it
were incidental. Their chief work is to time this great
clock, our earth, and so to trace the motions of those
celestial indices which afford our fundamental time-
measures, as to ensure as far as possible the safety of
our navy, royal and mercantile.[1]

[1] This consideration has been altogether lost sight of in certain re-
cent propositions for extending government aid to astronomical inquiries
of another sort. It may be a most desirable thing that Government
should find means for inquiring into the physical condition of sun and
moon, planets and comets, stars and all the various orders of star-clusters.

Let us see how this is brought about, not indeed inquiring into the processes by which at the Greenwich Observatory the elements of safety are obtained, but considering the method by which a seaman makes use of those elements.

In the measures heretofore considered, the captain of a ship in reality relies on terrestrial measurements. He reasons that, having been on such and such a day in a given place, and having in the interval sailed so many miles in such and such directions, he must at the moment be in such and such a place. This is called Navigation. In the processes next to be considered, which constitute a part of the science of Nautical Astronomy, the seaman trusts to celestial observations independent of all terrestrial measurements.

The points to be determined by the voyager are his latitude and longitude. The latitude is the distance north or south of the equator, and is measured always from the equator in degrees, the distance from equator to pole being divided into ninety equal parts, each of which is a degree.[1]   The longitude is the distance east

But if such matters are to be studied at Government expense, it should be understood that the inquiry is undertaken with the sole purpose of advancing our knowledge of these interesting subjects, and should not be brought into comparison with the utilitarian labours for which our Royal Observatory was founded.

[1] Throughout this explanation all minuter details are neglected. In reality, in consequence of the flattening of the earth's globe, the degrees of latitude are not equal, being larger the farther we go from the equator. Moreover, strictly speaking, it is incorrect to speak of distances being divided into degrees, or to say that a degree of latitude or longitude contains so many miles; yet it is so exceedingly inconvenient to employ any other way of speaking in popular description, that I trust any

or west of Greenwich (in English usage, but other nations employ a different starting-point for measuring longitudes from). Longitude is not measured in miles, but in degrees. The way of measuring is not very readily explained without a globe or diagrams, but may be thus indicated :— Suppose a circle to run completely round the earth, through Greenwich and both the poles; now if this circle be supposed free to turn upon the polar axis, or on the poles as pivots, and the half which crosses Greenwich be carried (the nearest way round) till it crosses some other station, then the arc through which it is carried is called the longitude of the station, and the longitude is easterly or westerly according as this half-circle has to be shifted towards the east or west. A complete half-turn is 180 degrees, and by taking such a half-turn either eastwardly or westwardly, the whole surface of the earth is included. Points which are 180 degrees east of Greenwich are thus also 180 degrees west of Greenwich.

So much is premised in the way of explanation to make the present paper complete; but ten minutes' inspection of an ordinary terrestrial globe will show the true meaning of latitude and longitude more clearly (to those who happen to have forgotten what they learned at school on these points) than any verbal description.

Now it is sufficiently easy for a sea-captain in fine weather to determine his latitude. For places in dif-

astronomers or mathematicians who may read this article, will forgive the solecism.

ferent latitudes have different celestial scenery, if one
may so describe the aspect of the stellar heavens by
night and the apparent path of the sun by day. The
height of the pole-star above the horizon, for instance,
at once indicates the latitude very closely, and would
indicate the latitude exactly if the pole-star were
exactly at the pole instead of being merely close to it.
But the height of any known star when due south also
gives the latitude. For at every place in a given lati-
tude, a star rises to a given greatest height when due
south; if we travel farther south the star will be higher
when due south; if we travel farther north it will be
lower; and thus its observed height shows just how far
north of the equator any northerly station is, while if
the traveller is in the southern hemisphere correspond-
ing observations show how far to the south of the
equator he is.

But commonly the seaman trusts to observation of
the sun to give him his latitude. The observation is
made at noon, when the sun is highest above the
horizon. The actual height is determined by means of
the instrument called the sextant. This instrument
need not be here described; but thus much may be
mentioned to explain that process of taking the sun's
meridian latitude which no doubt everyone has wit-
nessed who has taken a long sea-journey. The sextant
is so devised that the observer can see two objects at
once, one directly and the other after reflection of its
light; and the amount by which he has to move a cer-
tain bar carrying the reflecting arrangement, in order
to bring the two objects into view in the same direction,

shows him the real divergence of lines drawn from his.
eye to the two objects. To take the sun's altitude
then with this instrument, the observer takes the sun
as one object and the horizon directly below the sun as
the other : he brings them into view together, and then
looking at the sextant to see how much he has had to.
move the swinging arm which carries the reflecting
glasses, he learns how high the sun is. This being
done at noon, with proper arrangements to ensure that
the greatest height then reached by the sun is observed,
at once indicates the latitude of the observer. Suppose,.
for example, he finds the sun to be forty degrees above
the horizon, and the Nautical Almanac tells him that
at the time the sun is ten degrees north of the celestial
equator, then he knows that the celestial equator is
thirty degrees above the southern horizon. The pole
of the heavens is therefore sixty degrees above the
northern horizon, and the voyager is in sixty degrees
north latitude. Of course, in all ordinary cases, the
number of degrees is not exact, as I have here for sim-
plicity supposed, and there are some niceties of obser-
vation which would have to be taken into account, in
real work. But the principle of the method is suffici-
ently indicated by what has been said, and no useful
purpose could be served by considering minutiæ.

Unfortunately, the longitude is not determined so-
readily. The very circumstance which makes the de--
termination of the latitude so simple introduces the
great difficulty which exists in finding the longitude.
I have said that all places in the same latitude have the-
same celestial scenery; and precisely for this reason it

is difficult to distinguish one such place from another, that is, to find on what part of its particular latitude-circle any place may lie.

If we consider, however, how longitude is measured, and what it really means, we shall readily see where a solution of the difficulty is to be sought. The latitude of a station means how far towards either pole the station is; its longitude means how far *round* the station is from some fixed longitude. But it is by turning round on her axis that the earth causes the changes which we call day and night; and therefore these must happen at different times in places at different distances round. For example, it is clear that if it is noon at one station it must be midnight at a station half-way round from the former. And if anyone at one station could telegraph to a person at another, ' It is exactly noon here,' while this latter person knew from his clock or watch, that it was exactly midnight where *he* was, then he would know that he was half-way round exactly. He would, in fact, know his longitude from the other station. And so with smaller differences. The earth turns we know from west to east,—that is, a place lying due west of another is so carried as presently to occupy the place which its easterly neighbour had before occupied, while this last place has gone farther east yet. Let us suppose an hour is the time required to carry a westerly station to the position which had been occupied by a station to the east of it. Then manifestly every celestial phenomenon depending on the earth's turning will occur an hour later at the

westerly station. Sunrise and sunset are phenomena
of this kind. If I telegraph to a friend at some station
far to the west, but in the same latitude, 'The sun is
rising here,' and he finds that he has to wait exactly an
hour before the sun rises there, then he knows that he
is one hour west of me in longitude, a most inexact yet
very convenient and unmistakable way of speaking.
As there are twenty-four hours in the day, while a com-
plete circle running through my station and his (and
everywhere in the same latitude) is supposed to be
divided into 360 degrees, he is 15 degrees (a 24th part
of 360) west of me; and if my station is Greenwich,
he is in what we, in England, call 15 degrees west
longitude.[1]

But what is true of sunrise and sunset in the same
latitudes and in different longitudes, is true of noon
whatever the latitude may be. And of course it is true
of the southing of any known star. Only unfortunately
one cannot tell the exact instant when either the sun
or a star is due south or at its highest above the horizon.
Still, speaking generally, and for the moment limiting
our attention to noon, every station towards the west
has noon later, while every station towards the east has
noon earlier, than Greenwich (or whatever reference-
station is employed).

I shall presently return to the question how the

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[1] In this case, he is 'at sea' (which, I trust, will not be the case
with the reader), and, we may suppose, connected with Greenwich by a
submarine telegraph in course of being laid. In fact, the position of the
Great Eastern throughout her cable-laying journeys, was determined by
a method analogous to that sketched above.

longitude is to be determined with sufficient exactness
for safety in sea voyages. But I may digress here to
note what happens in sea voyages where the longitude
changes. If a voyage is made towards the west, as from
England to America, it is manifest that a watch set to
Greenwich time, will be in advance of the local time as
the ship proceeds westwards, and will be more and
more in advance the farther the ship travels in that
direction. For instance, suppose a watch shows Green-
wich time; then when it is noon at Greenwich the
watch will point to twelve, but it will be an hour before
noon at a place fifteen degrees west of Greenwich, two
hours before noon at a place thirty degrees west, and so
on : that is, the watch will point to twelve when it is
only eleven o'clock, ten o'clock, and so on, of local time.
On arrival at New York, the traveller would find that
his watch was nearly five hours fast. Of course the
reverse happens in a voyage towards the east. For
instance, a watch set to New York time would be found
to be nearly five hours slow, for Greenwich time, when
the traveller arrived in England.

In the following passage these effects are humour-
ously illustrated by Mark Twain,—

'Young Mr. Blucher, who is from the Far West,
and on his first voyage' (from New York to Europe)
"was a good deal worried by the constantly changing
"ship-time." He was proud of his new watch at first,
and used to drag it out promptly when eight bells
struck at noon, but he came to look after a while as if
he were losing confidence in it. Seven days out from

New York he came on deck, and said with great decision, "This thing's a swindle!" "What's a swindle?" "Why, this watch. I bought her out in Illinois—gave 150 dollars for her, and I thought she was good. And, by George, she *is* good on shore, but somehow she don't keep up her lick here on the water—gets sea-sick, may-be. She skips; she runs along regular enough till half-past eleven, and then all of a sudden she lets down. I've set that old regulator up faster and faster, till I've shoved it clean round, but it don't do any good; she just distances every watch in the ship,[1] and clatters along in a way that's astonishing till it's noon, but them 'eight bells' always gets in about ten minutes ahead of her any way. I don't know what to do with her now. She's doing all she can,—she's going her best gait, but it won't save her. Now, don't you know there ain't a watch in the ship that's making better time than she is; but what does it signify? When you hear them 'eight bells,' you'll find her just ten minutes short of her score—sure." The ship was gaining a full hour every three days, and this fellow was trying to make his watch go fast enough to keep up to her. But, as he had said, he had pushed the regulator up as far as it would go, and the watch was "on its best gait," and so nothing was left him but to fold his hands and see the ship beat the race. We sent him to the captain, and he explained to him the mystery of "ship time" and

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[1] Because *set* to go 'fast.' Of course, the other watches on board would be left to go at their usual rate, and simply put forward at noon each day by so many minutes as corresponded to the run eastwards since the preceding noon.

set his troubled mind at rest. This young man,' proceeds Mr. Clemens, *apropos des bottes,* 'had asked a great many questions about sea-sickness before we left, and wanted to know what its characteristics were, and how he was to tell when he had it. He found out.'

I cannot leave Mark Twain's narrative, however, without gently criticising a passage in which he has allowed his imagination to invent effects of longitude which assuredly were never perceived in any voyage since the ship ' Argo' set out after the Golden Fleece. 'We had the phenomenon of a full moon,' he says, ' located just in the same spot in the heavens, at the same hour every night. The reason of this singular conduct on the part of the moon did not occur to us at first, but it did afterwards, when we reflected that we were gaining about twenty minutes every day, because we were going east so fast, we gained just about enough every day to keep along with the moon. It was becoming an old moon to the friends we had left behind us, but to us Joshuas it stood still in the same place, and remained always the same.' Oh, Mr. Clemens, Mr. Clemens! In a work of imagination (as the ' Innocents Abroad' must, I suppose, be to a great extent considered), a mistake such as that here made is perhaps not a very serious matter: but suppose some unfortunate compiler of astronomical works should happen to remember this passage, and to state (as a compiler would be tolerably sure to do, unless he had a mathematical friend at his elbow), that by voyaging eastwards at such and such a rate, a traveller can always have the

moon 'full' at night, in what an unpleasant predica-
ment would the mistake have placed him. Such things
happen, unfortunately; nay, I have even seen works, in
which precisely such mistakes have been made, in use
positively as textbooks for examinations. On this ac-
count, our fiction writers must be careful in introduc-
ing science details, lest peradventure science teachers
(save the mark!) be led astray.

It need scarcely be said that no amount of east-
wardly voyaging would cause the moon to remain
always 'full' as seen by the voyager. The moon's
phase is the same from whatever part of the earth she
may be seen, and she will become 'new,' that is, pass
between the earth and the sun, no matter what voyages
may be undertaken by the inhabitants of earth. Mr.
Clemens has confounded the monthly motion of the
moon with her daily motion. A traveller who could
only go fast enough eastwards might keep the moon
always due south. To do this he would have to travel
completely round the earth in a day and (roughly)
about $50\frac{1}{2}$ minutes. If he continued this for a whole
month, the moon would never leave the southern hea-
vens; but she would not continue 'full.' In fact we
see that the hour of the day (local time) would be con-
tinually changing,—since the traveller would not go
round once in twenty-four hours (which would be fol-
lowing the sun, and would cause the hour of the day to
remain always the same) but in twenty-four hours and
the best part of another hour; so that the day would

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seem to pass on, though very slowly, lasting a lunar
month instead of a common day.

Everyone who makes a long sea-voyage must have
noted the importance attached to noon observations;
and many are misled into the supposition that these ob-
servations are directly intended for the determination
of the longitude (or, which is the same thing in effect,
for determining true ship-time). This, however, is a
mistake. The latitude can be determined at noon, as
we have seen. A rough approximation to the local
time can be obtained, and is commonly obtained, by
noting when the sun begins to dip after reaching the
highest part of his course above the horizon. But this
is necessarily *only* a rough approximation, and quite
unsuited for determining the ship's longitude. For the
sun's elevation changes very slowly at noon, and no dip
can be certainly recognized even from *terra firma*, far
less from a ship, within a few minutes of true noon. A
determination of time effected in this way, serves very
well for the ship's ' watches,' and accordingly when the
sun, so observed, begins to dip, they strike 'eight bells'
and ' make it noon.' But it would be a serious matter
for the crew if that was made the noon for working the
ship's place; for an error of many miles would be in-
evitable.

The following passage from ' Foul Play,' illustrates
the way in which mistakes have arisen on this point.
The hero, who being a clergyman and a university man
is of course a master of every branch of science, is about
to distinguish himself before the heroine by working

out the position of the ship *Proserpine*, whose captain is senselessly drunk. After ten days' murky weather ' the sky suddenly cleared, and a rare opportunity oc-curred to take an observation. Hazel suggested to Wylie, the mate, the propriety of taking advantage of the moment, as the fog bank out of which they had just emerged would soon envelop them again, and they had not more than an hour or so of such observation avail-able. The man gave a shuffling answer. So Hazel sought the captain in his cabin. He found him in bed. He was dead drunk. On a shelf lay the instruments. These Hazel took and then looked round for the chro-nometers. They were safely locked in their cases. He carried the instruments on deck, together with a book of tables, and quietly began to make preparations, at which Wylie, arresting his walk, gazed with utter as-tonishment ' (as well he might).

' " Now, Mr. Wylie, I want the key of the chrono-meter cases."

' " Here is a chronometer, Mr. Hazel," said Helen, very innocently, " if that is all you want."

' Hazel smiled, and explained that a ship's clock is made to keep the most exact time ; that he did not re-quire the time of the spot where they were, but Green-wich time. He took the watch, however. It was a large one for a lady to carry ; but it was one of Frod-sham's masterpieces.

' " Why, Miss Rolleston," said he, " this watch must be two hours slow. It marks ten o'clock; it is now nearly midday. Ah, I see," he added with a smile,

" you have wound it regularly every day, but you have forgotten to set it daily. Indeed, you may be right ; it would be a useless trouble, since we change our longitude hourly. Well, let us suppose that this watch shows the exact time at Sydney, as I presume it does, I can work the ship's reckoning from that meridian, instead of that of Greenwich." And he set about doing it.' Wylie, after some angry words with Hazel, brings the chronometers and the charts. Hazel ' verified Miss Rolleston's chronometer, and allowing for difference of time, found it to be accurate. He returned it to her, and proceeded to work on the chart. The men looked on : so did Wylie. After a few moments, Hazel read as follows : West longitude 146° 53′ 18″, South latitude 35° 24′. The island of Opara [1] and the Four Crowns distant 420 miles on the N.N.E.,' and so on. And, of course, ' Miss Rolleston fixed her large soft eyes on the young clergyman with the undisguised admiration a woman is apt to feel for what she does not understand.'

The scene here described corresponds pretty closely, I have little doubt, with one actually witnessed by the novelist, except only that the captain or chief officer made the observations, and that either there had not been ten days' murky weather or else that in the forenoon, several hours at least before noon, an observation of the sun had been made. The noon observation

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[1] The island fixes the longitude at about 147°, otherwise I should have thought the 4 was a misprint for 7. In longitude 177° west, Sydney time would be about 2 hours slow, but about 4 hours slow in longitude 147° west.

would give the latitude, and combined with a forenoon observation, would give the longitude ; but *alone* would be practically useless for that purpose. It is curious that the novelist sets the longitude as assigned much more closely than the latitude, and the value given would imply that the ship's time was known within less than a second. This would in any case be impracticable; but from noon observations the time could not be learned within a minute at the least. The real fact is, that to determine true time, the seaman selects, not noon, as is commonly supposed, but a time when the sun is nearly due east, or due west. For then the sun's elevation changes most rapidly, and so gives the surest means of determining the time. The reader can easily see the *rationale* of this, by considering the case of an ordinary clock-hand. Suppose our only means of telling the time was by noting how high the end of the minute hand was; then clearly we should be apt to make a greater mistake in estimating the time when the hand was near XII, than at any other time, because then its end changes very slowly in height, and a minute more or less makes very little difference. On the contrary, when the hand was near III and IX, we could in a very few seconds note any change of the height of its extremity. In one case we could not tell the time within a minute or two ; in the other, we could tell it within a few seconds.

But the noon observation would be wanted to complete the determination of the longitude ; for until the latitude was known, the captain would not be aware

what apparent path the sun was describing in the heavens, and, therefore, would not know the time corresponding to any particular solar observation. So that a passenger, curious in watching the captain's work, would be apt to infer that the noon observations gave the longitude, since he would perceive that from them the captain worked out both the longitude and the latitude.

It is curious that another and critical portion of the same entertaining novel, is affected by the mistake of the novelist on this subject. After the scuttling of the *Proserpine*, and other events, Hazel and Miss Rolleston are alone on an island in the Pacific. Hazel seeks to determine their position, as one step towards escape. Now, ' you must know that Hazel, as he lay on his back in the boat, had often in a half-drowsy way, watched the effect of the sun upon the boat's mast: it now stood, a bare pole, and at certain hours acted like the needle of a dial, by casting a shadow on the sands. Above all, he could see pretty well, by means of this pole and its shadow, when the sun attained its greatest elevation. He now asked Miss Rolleston to assist him in making this observation exactly. She obeyed his instructions, and the moment the shadow reached its highest angle and showed the minutest symptom of declension, she said " Now," and Hazel called out in a loud voice ' (why did he do that?) ' " Noon !" " And forty-nine minutes past eight at Sydney," said Helen, holding out her chronometer; for she had been sharp enough to get it ready of her own accord. Hazel looked at her and at

the watch with amazement and incredulity. "What?" said he, "Impossible. You can't have kept Sydney time all this while." "And pray why not?" said Helen. "Have you forgotten that some one praised me for keeping Sydney time? it helped you, somehow or other, to know where we were." ' After some discussion in which she shows how natural it was that she should have wound up her watch every night, even when 'neither of them expected to see the morning,' she asks to be praised. ' "Praised!" cried Hazel, excitedly, "worshipped, you mean. Why, we have got the longitude by means of your chronometer. It is wonderful! It is providential. It is the finger of Heaven. Pen and ink, and let me work it out." ' He was 'soon busy calculating the longitude of Godsend Island.' What follows is even more curiously erroneous. ' "There," said he. "Now the latitude I must guess at by certain combinations. In the first place the slight variation in the length of the days. Then I must try and make a rough calculation of the sun's parallax." ' (It would have been equally to the purpose to have calculated how many cows' tails would reach to the moon.) ' "And then my botany will help me a little; spices furnish a clue; there are one or two that will not grow outside the tropic," ' and so on. He finally sets the latitude between the 26th and 33rd parallels, a range of nearly 500 miles. The longitude, however, which is much more closely assigned, is wrong altogether, being set at $103\frac{1}{2}$ degrees *west*, as the rest of the story requires. For Godsend Island is within not many days'

sail of Valparaiso. The mistake has probably arisen
from setting Sydney in *west* longitude instead of *east*
longitude, 151° 14′; for the difference of time, 3h. 11m.,
corresponds within a minute to the difference of longi-
tude between 151° 14′ west and 103½° west.

Mere mistakes of calculation, however, matter little
in such cases. They do not affect the interest of a
story even in such extreme cases as in 'Ivanhoe,' where
a full century is dropped (in such sort that one of
Richard the First's knights holds converse with a contem-
porary of the Conqueror, who, if my memory deceives
me not, was Cœur de Lion's great-great-grandfather).
It is a pity, however, that a novelist or indeed any
writer should attempt to sketch scientific *methods* with
which he is not familiar. No discredit can attach to
any person, not an astronomer, who does not understand
the astronomical processes for determining latitude and
longitude, any more than to one who, not being a
lawyer, is unfamiliar with the rules of Conveyancing.
But when an attempt is made by a writer of fiction to
give an exact description of any technical matter, it is
as well to secure correctness by submitting the descrip-
tion to some friend acquainted with the principles of
the subject. For, singularly enough, people pay much
more attention to these descriptions when met with in
novels than when given in textbooks of science, and
they thus come to remember thoroughly well precisely
what they ought to forget. I think, for instance, that
it may not improbably have been some recollection of
'Foul Play' which led Mr. Lockyer to make the sur-

prising statement that longitude is determined at sea
by comparing chronometer time with local time, which
is found 'at noon by observing, with the aid of a
sextant, when the sun is at the highest point of its
path.' Our novelists really must not lead the student
of astronomy astray in this manner.

It will be clear to the reader, by this time, that the
great point in determining the longitude, is to have
the true time of Greenwich or some other reference
station, in order that by comparing this time with ship
time, the longitude east or west of the reference station
may be ascertained. Ship time can always be deter-
mined by a morning or afternoon observation of the
sun, or by observing a known star when towards the
east or west, at which time the diurnal motion raises or
depresses it most rapidly. The latitude being known,
the time of day (any given day) at which the sun or a
star should have any particular altitude is known also,
and, therefore, conversely, when the altitude of the sun
or a star has been noted, the seaman has learned the
time of day. But to find Greenwich time is another
matter; and without Greenwich time, ship time teaches
nothing as to the longitude. How is the voyager at sea
or in desert places to know the exact time at Green-
wich or some other fixed station? We have seen that
chronometers are used for this purpose; and chronome-
ters are now made so marvellously perfect in construction
that they can be trusted to show true time within a few
seconds, under ordinary conditions. But it must not
be overlooked that in long voyages a chronometer, how-

ever perfect its construction, is more liable to get wrong
than at a fixed station. That it is continually tossed
and shaken is something, but is not the chief trial to
which it is exposed. The great changes of temperature
endured when a ship passes from the temperate latitudes
across the torrid zone to the temperate zone again, try a
chronometer far more severely than any ordinary form
of motion. And then it is to be noted that a very
insignificant time-error corresponds to a difference of
longitude quite sufficient to occasion a serious error in
the ship's estimated position. For this reason and for
others, it is desirable to have some means of deter-
mining Greenwich time independently of chronometers.

This, in fact, is the famous problem for the solution
of which such high rewards were offered and have been
given.[1] It was to solve this problem that Whiston,
the same who fondly imagined Newton was afraid of
him,[2] suggested the use of bombs and mortars; for which
Hogarth pilloried him in the celebrated madhouse scene
of the Rake's Progress. Of course Whiston had perceived

[1] For the invention of the chronometer Harrison (a Yorkshire car-
penter and the son of a carpenter) received twenty thousand pounds.
This sum had been offered for a marine chronometer which would stand
the test of two voyages of assigned length. Harrison laboured fifty
years before he succeeded in meeting the required conditions.

[2] Newton, for excellent reasons, had opposed Whiston's election to
the Royal Society. Like most small men Whiston was eager to secure
a distinction which, unless spontaneously offered to him, could have
conferred no real honour. Accordingly he was amusingly indignant with
Newton for opposing him. 'Newton perceived,' he wrote, 'that I could
not do as his other darling friends did, that is, learn of him without
contradicting him when I differed in opinion from him : he could not in
his old age bear such contradiction, and so he was afraid of me the last
thirteen years of his life.'

the essential feature of all methods intended for deter-
mining the longitude. Any signal which is *recognis-
able*, no matter by eye or ear, or in whatsoever way, at
both stations, the reference station and the station
whose longitude is required, must necessarily suffice
to convey the time of one station to the other. The
absurdity of Whiston's scheme lay in the implied
supposition that any form of ordnance could propel
rocket signals far enough to be seen or heard in mid-
ocean. Manifestly the only signals available, when
telegraphic communication is impossible, are signals in
the celestial spaces; for these alone can be discerned
simultaneously from widely distant parts of the earth.
It has been to such signals, then, that men of science
have turned for the required means of determining
longitude.

Galileo was the first to point out that the satellites
of Jupiter supply a series of signals which might serve
to determine the longitude. When one of these
bodies is eclipsed in Jupiter's shadow, or passes out of
sight behind Jupiter's disc, or reappears from eclipse or
occultation, the phenomenon is one which can be seen
from a whole hemisphere of the earth's surface. It is
as truly a signal as the appearance or disappearance of
a light in ordinary night-signalling. If it can be cal-
culated beforehand that one of these events will take
place at any given hour of Greenwich time, then, from
whatever spot the phenomenon is observed, it is known
there that the Greenwich hour is that indicated.
Theoretically this is a solution of the famous problem;

and Galileo, the discoverer of Jupiter's four satellites,
thought he had found the means of determining the
longitude with great accuracy.   Unfortunately these
hopes have not been realised.   At sea, indeed, except
in the calmest weather, it is impossible to observe the
phenomena of Jupiter's satellites, simply because the
telescope cannot be directed steadily upon the planet.
But even on land Jupiter's satellites afford but imper-
fect means of guessing at the longitude.   For, at
present, their motions have not been thoroughly mas-
tered by astronomers, and though the Nautical Almanac
gives the estimated epochs for the various phenomena
of the four satellites, yet, owing to the imperfection of
the tables, these epochs are often found to be appreciably
in error.   There is yet another difficulty.   The satel-
lites are not mere points, but being in reality as large
as or larger than our moon, they have discs of appre-
ciable though small dimensions.   Accordingly they do
not vanish or reappear instantaneously, but gradually,
the process lasting in reality several seconds (a longer
or shorter time according to the particular satellite
considered), and the estimated moment of the pheno-
menon thus comes to depend on the power of the
telescope employed, on the skill or the visual powers of
the observer, on the condition of the atmosphere, and
so on.   Accordingly, very little reliance could be placed
on such observations as a means for determining the
longitude with any considerable degree of exactness.

No other celestial phenomena present themselves

except those depending on the moon's motions.[1] All
the planets, as well as the sun and moon, traverse at

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[1] If but one star or a few would periodically (and quite regularly)
'go out' for a few moments, the intervals between such vanishings being
long enough to ensure that one would not be mistaken in point of time
for the next or following one, then it would be possible to determine
Greenwich or other reference time with great exactness. And here we
cannot but recognize an argument against the singular theory that the
stars were intended simply as lights to adorn our heavens and to be of
use to mankind. The teleologists who have adopted this strange view,
can hardly show how the theory is consistent with the fact that quite
readily the stars (or a few of them) might have been so contrived as to
give man the means of travelling with much more security over the
length and breadth of his domain than is at present possible. In this
connection I venture to quote a passage in which Sir John Herschel has
touched on the *usefulness* of the stars, in terms which were they not
corrected by other and better known passages in his writings, might
suggest that he had adopted the theory I have just mentioned :—' The
stars,' he said, in an address to the Astronomical Society, in 1827, ' are
landmarks of the universe ; and amidst the endless and complicated
fluctuations of our system, seem placed by its Creator as guides and
records, not merely to elevate our minds by the contemplation of what is
vast, but to teach us to direct our actions by reference to what is im-
mutable in His works. It is indeed hardly possible to over-appreciate
their value in this point of view. Every well-determined star, from the
moment its place is registered, becomes to the astronomer, the
geographer, the navigator, the surveyor, a point of departure which can
never deceive or fail him,—the same for ever and in all places, of a
delicacy so extreme as to be a test for every instrument yet invented by
man, yet equally adapted for the most ordinary purposes ; as available
for regulating a town clock as for conducting a navy to the Indies ; as
effective for mapping down the intricacies of a petty barony, as for
adjusting the boundaries of transatlantic empires. When once its
place has been thoroughly ascertained, and carefully recorded, the
brazen circle with which the useful work was done may moulder, the
marble pillar may totter on its base, and the astronomer himself sur-
vive only in the gratitude of posterity ; but the record remains, and
transfuses all its own exactness into every determination which takes it
for a groundwork, giving to inferior instruments, nay, even to tem-
porary contrivances, and to the observations of a few weeks or days,

various rates and in different paths the sphere of the
fixed stars. But the moon alone moves with sufficient
rapidity to act as a time-indicator for terrestrial voya-
gers. It is hardly necessary to explain why rapidity of
motion is important; but the following illustration
may be given for the purpose. The hour-hand of a
clock does in reality indicate the minute as well as the
hour; yet owing to the slowness of its motion we
regard the hour-hand as an unsatisfactory time-indica-
tor, and only consider it as showing what hour is in
progress. So with the more slowly-moving celestial
bodies. They would serve well enough, at least some
among them would, to show the *day of the year*, if we
could only imagine that such information were ever
required from celestial bodies. But it would be hope-
less to attempt to ascertain the true time with any
degree of accuracy from their motions. Now the
moon really moves with considerable rapidity among

all the precision attained originally at the cost of so much time, labour,
and expense.' It is only necessary as a corrective to the erroneous ideas
which might otherwise be suggested by this somewhat high-flown pas-
sage, to quote the following remarks from the work which represented
Sir John Herschel's more matured views, his well-known ' Outlines of
Astronomy.' ' For what purpose are we to suppose such magnificent
bodies scattered through the abyss of space? Surely not to illuminate
our nights, which an additional moon of the thousandth part of the size
of our own world would do much better; nor to sparkle as a pageant
void of meaning and reality, and bewilder us among vain conjectures.
Useful, it is true, they are to man as points of exact and permanent
reference, but he must have studied astronomy to little purpose, who can
suppose man to be the only object of his Creator's care; or who does
not see in the vast and wonderful apparatus around us, provision for
other races of animated beings.'

the stars.[1] She completes the circuit of the celestial sphere in $27\frac{1}{3}$ days (a period less than the common lunation), so that in one day she traverses about thirteen degrees,—or her own diameter (which is rather more than half a degree) in about an hour. This, astronomically speaking, is very rapid motion ; and as it can be detected in a few seconds by telescopic comparison of the moon's place with that of some fixed star, it serves to show the time within a few seconds, which is precisely what is required by the seaman. Theoretically, all he has to do, is to take the moon's apparent distance from a known star, and also her height and the star's height above the horizon. Thence he can calculate what would be the moon's distance from the star at the moment of observation, if the observer were at the earth's centre. But the Nautical Almanac informs him of the precise instant of Greenwich time corresponding to this calculated distance. So he has, what he requires, the true Greenwich time.

It will be manifest that all methods of finding the way at sea, except the rough processes depending on the log and compass, require that the celestial bodies, or some of them, should be seen. Hence it is that cloudy weather for any considerable length of time, occasions danger and sometimes leads to shipwreck and loss of life. Of course the captain of a ship proceeds with extreme caution when the weather has long been

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[1] It was this doubtless which led to the distinction recognised in the book of Job, where the moon is described as ' *walking* in brightness.'

cloudy, especially if according to his reckoning he is drawing near shore. Then the lead comes into play, that by soundings, if possible, the approach to shore may be indicated. Then also by day and night a careful watch is kept for the signs of land. But it sometimes happens that despite all such precautions a ship is lost; for there are conditions of weather which, occurring when a ship is nearing shore, render the most careful look-out futile. These conditions may be regarded as included among ordinary sea-risks, by which term are understood all such dangers as would leave a captain blameless if shipwreck occurred. It would be well if no ships were ever lost save from ordinary sea-risks; but unfortunately ships are sometimes cast ashore for want of care; either in maintaining due watch as the shore is approached, or taking advantage of opportunities, which may be few and far between, for observing sun or moon, or stars, as the voyage proceeds. It may safely be said that the greater number of avoidable shipwrecks have been occasioned by the neglect of due care in finding the way at sea.

(From the *St. Paul's Magazine.*)

JOURNEYS TOWARDS THE NORTH POLE.

THERE is always a certain charm about the unknown. It is a peculiarity of man's nature to expect more from

what remains to be done than he has gained in past
achievements.   The Elizabethan traveller sailing from
place to place in search of El Dorado ; Palissy selling
his last stick of furniture to try new combinations in
pottery, always confident that the latest would reward
him for past failures ; Kepler spending twenty years
of his life—amidst poverty and heavy domestic afflic-
tions—in trying new arrangements of figures to 'har-
monise ' (as he termed it) the celestial motions ;—these,
and a thousand more which might be quoted, are
instances of the operation of that unquenchable fire of
hope which animates the human race.   But there is
another feeling equally strong in man—the desire to do
what no one else has been able to do, to go where no
human foot has ever trod, to attain what other men
have agreed to look upon as unattainable.

Both these feelings seem to invite the hardy mari-
ner within the Arctic domains.   He may not, indeed,
hope to find in those bleak and uninviting regions the
productions—mineral, animal, and vegetable—which
constitute the wealth of nations.   But there are not
wanting tangible rewards held out (in promise, at least)
to the Arctic explorer; and the admiration of mankind,
which has been freely given to those who have explored
the sources of the Nile, or traversed the Australian
deserts, or discovered the North-west Passage, or
tracked the magnetic meridians to their pole, will
certainly not be withheld from the man who shall lead
the way to the North Pole of the earth.

Arctic expeditions have been more intimately asso-

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ciated with the discovery of America than many sup-
pose.   When Columbus set forth on his first journey,
his object was not the exploration of new regions.
He promised his followers that he would 'find the east
in the west.'   Deceived by the chart of the Florentine
physician and astronomer Toscanelli, he was under the
impression that Japan (Zip-angu he called it) lay
nearer to Europe than the western shores of America
actually lie, and when on the 12th of October, 1492,
he landed at Guanahani, he was persuaded that he had
reached the East Indian Archipelago.   Subsequent
voyages having shown that a vast continent extends
from north to south as a barrier against that westerly
journey to Asia which had so great an attraction for the
sailors of the fifteenth and sixteenth centuries, they
sailed northwards with the object of rounding the
northern boundary of the continent.   Thus Cabot, the
discoverer of the mainland of America, set forth on a
second journey with the avowed object of reaching
China by a north-west passage.   In the attempt he
penetrated as far north as the Arctic Circle, and the
notion then occurred to him of making a journey to
the North Pole itself (a lo del polo arctico) at some
future period.   Since that time nearly two hundred
vessels, besides boat-and-sledge expeditions, have visited
Arctic regions, with different plans and objects, but
never losing sight of the promised western route to
India.

We must not omit to notice however, in passing,
that not only had northern journeys been undertaken

before the time of Cabot, but the American mainland had been discovered by Scandinavian voyagers, who passed from Iceland to Greenland, and thence to Labrador. There is also a tradition of the discovery of America from Ireland before the year 1000. ‘According to testimonies which go back as far as 1064,’ says Humboldt, ‘Ari Marsson, of the powerful Icelandic family of Ulf the Squint-eyed, was driven by storms to the coasts between Virginia and Florida, designated by the name of White Men's Land, and peopled from Ireland.’

The attempts to discover a north-west passage were continued long after it had become abundantly evident that such a passage would be useless to traders. The first English expedition had sailed in 1553 under Hugh Willoughby. In 1607 Hudson sailed northwards as far as latitude 81° 30′ on the open sea between Greenland and Spitzbergen. Then Cook, Hearne, and Mackenzie followed; and after an interval of many years Ross and Parry made successful journeys, though the north-west passage still continued undiscovered. Parry's first independent expedition in 1819, in which he reached as far west as Melville Island, sufficed to prove how utterly unsuitable a passage round the northern shores of America must be for trading ships. Yet Franklin and Richardson in the following year started on a land journey, with the object of exploring the Arctic coasts of North America; and again, a few years later, the same gallant leaders, accompanied by Back, headed a second overland expedition, which explored the coast between the Coppermine

and Mackenzie Rivers.   Then Beechey sailed by Beh-
ring's Straits, and Dease and Simpson in boat-voyages
completed the survey of the northern shores of America.
And though now, for awhile, the spirit of polar adven-
ture slumbered, 'there still existed ardent believers in
the north-west passage, and foremost amongst those who
were willing to peril their lives in the solution of a.
problem which had engaged the attention of English-
men for three centuries was Franklin, the early hero of
Arctic adventure.'

In May 1845 Franklin started on his last unfor-
tunate voyage.   He left England determined that, if it
were possible by any effort he could make, he would
achieve the north-west passage.   In August 1845 his
party were seen in the northern part of Baffin's Bay.
It was next learnt that they had passed the winter of
1845-46 beside Cape Riley, near Wellington Channel.
But whither they went afterwards has never been
certainly known.   In 1854 Dr. Rae brought to England
a number of articles, which had undoubtedly belonged
to the Franklin expedition.   He had obtained these
from Esquimaux in the neighbourhood of Back's River,
the outlet of Garry Lake.   Franklin and his gallant
company, one hundred and thirty-eight in all, had
perished—*how* is unknown, but almost certainly on
land.   There seems too much reason to fear that
Franklin's experience in overland Arctic journeying
had tempted him to forsake his ship when he found her
ice-bound in the Arctic channel.   Had he remained in
her, a powerful agent of whose effects he had had no

experience,—the great annual outdrift—would, in all probability, have carried him and his companions into safety.

Later expeditions were sent out rather with the hope of saving Franklin than with the object of effecting geographical discoveries. The voyage of the *Investigator*, under Captain Maclure, in 1850-53, led, however, to the final solution of the problem which had so long engaged men's attention. Passing through Behring's Straits Maclure sailed to the eastward, and wintered on the shores of Baring's Island. The two next winters (1851-52 and 1852-3) were passed on the northern shore, where the *Investigator* had become fixed in the ice. She was afterwards abandoned, and a party of the officers and crew crossed the ice to the southern side of Melville Island, the point reached from the westward by Parry in 1820. Thus it was proved that a continuous channel exists between the Atlantic and the Pacific. The fruit which had seemed so enticing to the eye was gathered, but its contents proved to be little better than dust and ashes.

In the meantime, however, there had sprung up a desire to carry exploration to the neighbourhood of the North Pole. We have seen that Hudson had reached a very high latitude in 1607. Scoresby and others had sailed far on the same track, and it was found that open sea, more than a mile in depth, extended far to the north between Greenland and Spitzbergen. In 1827 Sir Edward Parry attempted to reach the North Pole by way of Spitzbergen. He proposed to land on the ice,

and to prosecute his journey directly northwards in sledges drawn by Esquimaux dogs. I have not by me his account of the expedition, but so far as I can remember he made no use of the Esquimaux nor of their dogs, he and his crew starting without aid on their celebrated boat-and-sledge expedition. They advanced successfully for a while, and, though the journey was wearisome, they were encouraged by the progress they made, and the prospect of fame and reward, to persevere in their northward course. But soon Parry noticed a. suspicious circumstance. At the end of each day's. journey he made observations to determine the amount of their northward progress. He began to find that they were making less progress than he had expected from the rate at which they traversed the ice and the length of time during which they persevered each day in their exertions. Gradually the deficiency increased, until at length, at the end of one day's hard labour, he found that they had hardly maintained their place. *The whole ice-field was travelling southwards as fast as they travelled northwards.* It was, of course, useless to persevere under these circumstances. Parry therefore led his party home, after having attained lat. 82° 45' north, the most northerly yet reached. We shall see presently that Parry's failure has a very important bearing on the *theory* of Polar travel.

Dr. Kane of the United States navy followed another track, passing up Smith's Sound to the west of Green-land, and north of Baffin's Bay. He wintered in 1853-4, 1854-5 in Van Reusselaer's Inlet on the western

coast of Greenland, in lat. 78° 43'—'a higher latitude than had ever before formed the winter quarters of any explorer.' Here he abandoned his ship, the *Advance*, and travelled onwards to Uppernavik by a journey in boats over ice and water. He traced Kennedy Channel —the continuation of Smith's Sound—as far north as 81° 22'. Here land was seen to the north-west. He named the most northerly visible heights Parry Mountains, in honour of our English explorer. They lie farther north than any land yet seen. But the most remarkable result of Kane's journey was the discovery, that to the north-east of Kennedy Channel there extends, as far as the eye can reach, an open sea, 'rolling with the swell of a boundless ocean.'

My readers doubtless do not need to be reminded of Dr. Hayes' late journey, and of the evidence which it affords of an Arctic ocean extending far to the north of any regions yet reached.

So far I have followed evidence about which no doubt can be felt; but there are not wanting accounts of voyages in which the Pole is said to have been approached much more nearly. In the year 1818, Barrington and Beaufoy brought before the notice of scientific men the evidence of Dutch captains, who stated that they had approached within two or three degrees of the Pole, that they had there found a warm sea (compared with that to the west of Greenland), and a swell which proved that the sea was of wide extent. One of these witnesses stated that he had *passed* the North Pole. Many captains of whaling-ships have

stated that they have passed the Pole after penetrating the ice which lies to the north of Spitzbergen. It is doubtful how far these accounts are given in good faith, or, if so given, whether the men themselves were sufficiently familiar with the laws which govern the magnetic needle to estimate their true position.

Baron Wrangel tried a new route, starting north-wards from the coast of Siberia over vast ice-fields, which he supposed to extend far to the north. In *that* direction, also, open sea was found after the explorers had travelled a considerable distance.

Again, De Haven, when he went in command of the American expedition in search of Sir John Franklin, was told in his letter of instructions that when he had gone far up into Wellington Channel he was to look for an open sea to the northward and westward. He did so, and saw in that direction a 'water-sky.' Later, Captain Penny sailed upon open water there.

If we consider the journeys that have been already made towards the North Pole, we shall see that there is very little mystery about a 'new and untried route' recently proposed by M. Lambert. Mariners have jour-neyed towards the north—1, by way of the open sea which lies between Greenland and Spitzbergen ; 2, from Spitzbergen itself; 3, from the coasts of Siberia ; and 4, from the shores of North America. There is only one untried route, and that is the sea-voyage by Behring's Strait. When Captain Maclure passed up this strait, and had rounded the north-western shores of America, he sailed to the east, as we have seen, and no other

mariner, so far as we are aware, has sailed due north-
wards from Behring's Straits.

Lately, when Captain Osborn proposed that an
expedition should start for the North Pole, he sug-
gested that the course followed by Kane should be
tried again. It is well known that Kane's attempt
failed for want of sufficient means. Captain Osborn
pointed out that ' two small screw-steamers, with one
hundred and twenty officers and men, would not appear
as a large item amongst the fifty thousand men
annually voted for the navy ; and such employment
would be better training and less expensive of life and
money than small warlike preparations against Ashantee,
Japan, or New Zealand.'

Dr. Petermann about the same time proposed that
Hudson's route should be followed. Hudson sailed on the
open sea between Greenland and Spitzbergen ; and if so
high a latitude as Hudson attained could be reached in
a sailing vessel, it seemed reasonable to hope that a
steam-ship might be carried much farther in the same
direction.

Captain Osborn, on the contrary, urged that Kane's
route was preferable, because it afforded fixed points for
depositing provisions. ' Greenland is known to extend
one hundred and twenty miles nearer to the Pole than
Spitzbergen, and there is every probability that it runs
much farther north. As far as it has yet been traced
icebergs are observed to come down the coast, a sure
indication of extensive glaciers, which, like rivers,
their representatives in warmer regions, demand a great

extent of land in order to become large.' If land or
fixed ice really runs all the way to the North Pole,
there would be nine hundred and sixty-eight miles of
'sledging' in the journey from the Parry Mountains to
the Pole and back again, and more than this has often
been done by Arctic voyagers. 'Commander M'Clin-
tock's party in 1853 went 1,220 miles in one hundred
and five days; Commander G. Richards, 1,012 miles in
one hundred and two days; Lieutenant Mecham, 1,203
miles; Captains Richards and Osborn, 1,093 miles;
Lieutenant Hamilton, 1,150 miles, with a dog-sledge
and one man. In 1854 Lieutenant Mecham, 1,157
miles in only seventy days; Lieutenant Young, 1,150
miles; and Captain M'Clintock, 1,330 miles.'

It will be seen that Captain Osborn argued on the
supposition that the North Pole is occupied by land or
fixed ice, extending connectedly as far as the Parry Moun-
tains. On the other hand, Dr. Petermann believed that
the North Pole is occupied by an open sea, in summer
at least. What view M. Lambert took is uncertain;
but it seems probable that he hoped to attain to the
North Pole by sea-journeying only.

Whether the North Pole is always ice-bound or
occupied by a sea which is free from ice in summer,
has long been a question in dispute. The Russian
explorers are strong believers in the existence of a
Polynia, or open Polar ocean, extending northwards from
Spitzbergen. Captain Maury, who has given the sub-
ject a great deal of attention, believes also in the
existence of a Polar sea, respecting which, however, he

has expressed some rather bizarre views. It will be remembered that Dr. Kane found himself on the shore of an unbroken expanse of water. ' Its waves,' says. Captain Maury, ' were dashing on the beach with the swell of a boundless ocean. The tides ebbed and flowed in it, and I apprehend that the tidal wave from the Atlantic can no more pass under this icy barrier to be propagated in the seas beyond than the vibrations of a musical string can pass with its notes a fret on. which the musician has placed his finger. These tides,. therefore, must have been born in that cold sea, having their cradle about the North Pole.' He infers, therefore, that most if not all of the unexplored regions about the Pole are covered with deep water, but that. this water is so barred in by land or ice from the Atlantic and the Pacific, that the tides seen by Kane had not been propagated from either of those oceans,. but were generated in the Polar sea itself.

But this conclusion cannot be accepted. No one who is familiar with the astronomical doctrine of the tides can believe for a moment that tides could be generated in a landlocked ocean so limited in extent as. the North Polar sea (assuming its existence) must necessarily be. I say assuming its existence, meaning, *not* the existence of water around the North Pole or in its vicinity, but the existence of an ocean there, barred in on every side from the Atlantic and Pacific Oceans. Advances have been made towards the Pole, which show that if an ocean indeed exist so barred in, its extent must be comparatively small. Hudson advanced to.

within eight and a half degrees of the Pole on the open
Atlantic Ocean. Parry approached yet nearer from the
side of Spitzbergen; Penny sailed on an open sea com-
municating with Wellington Channel. Maclure showed
that there is open sea communicating between the
Pacific and Melville Island; and lastly, the Russian
explorers have shown that the Pacific communicates
with an open sea far to the north of Siberia. We have
not yet then even arrived at the solid barrier which is
to lock in Maury's supposed ocean. Giving to that
barrier any reasonable breadth, the ocean within cannot
exceed the Mediterranean in extent, and we know that
in the Mediterranean there is scarcely any perceptible
tide, and that what tide there is comes from the Atlantic
through the Straits of Gibraltar.

But it may be asked, How can we speak of Parry's
expedition as affording evidence of open sea when he
travelled on an ice-field? For this reason simply, that
his ice-field was floating, and not merely floating, but
travelling. With every allowance for a process of
melting taking place along its southern edge, and for
the formation of new ice at its northern edge, accom-
panied (let us grant) by a change of figure of the whole
field through compression in one part and dilatation in
another, yet it must be admitted that *under the field*
there was water, and that it could only have been here
and there that the ice 'felt' the solid ground. We
know what sort of process was in progress under the
field, since the face of Europe is *scored* and *striated*

with the passage over it, in long past ages, of precisely
such fields as that Parry travelled over.

But now consider the extent of Parry's ice-field.
On every side, as far as the eye could reach, was ice,
and there was not even a 'water-sky' visible. Had
there been, we know that Parry was prepared to have
sailed upon the open sea : and he would certainly have
adopted such a course, as almost certain to enable him
to travel much farther northward with comparative
ease. But this is not all. He had been travelling
northwards for two or three hundred miles, and the ice-
field had carried him back (if I remember rightly)
about a hundred and fifty miles. Hence the point he
actually reached had been much farther north when
Parry first started. And we learn, therefore, that *at
that northern point, scarcely five degrees from the
North Pole,* there is deep water, on which Parry's ice-
island was floating freely. So that, if there is a barrier
at all in that direction, it must lie within a few degrees
of the North Pole.

Combining Hudson's evidence of the existence of
open sea to within eight and a half degrees of the
North Pole on the eastern side of Greenland, and the
evidence afforded by Parry's floating ice-ship, with
Scoresby's deep-sea soundings in high latitudes, I feel
convinced that there exists open water in the prolonga-
tion of the Atlantic valley to within five degrees of
the Pole, and probably much nearer. A screw steamer
might well penetrate (after a few checks, no doubt,

from floating ice-fields) two or three hundred miles
farther north than Hudson in his old-fashioned caravel.
If we remember how Sir James Ross sailed round and
round the apparently impregnable ice-fortresses which
guard the Antarctic seas, and succeeded in penetrating
very much nearer to the southern magnetic pole than
he could have hoped for on a first view of his difficulties,
we may well hope that something similar might be
done in the northern seas.

But much more might be done. The tides seen by
Dr. Kane have to be accounted for; and what other
explanation can be offered than that they are derived
from the Atlantic, *not* by Smith's Sound—the only
course considered by Maury—but round the northern
shores of Greenland. Those tides spoke emphatically
of an open sea communication with the Atlantic.
Kane saw the water extending as far as the eye could
reach to the north-east. Hudson has shown that the
Atlantic stretches far to the north. Admitting, there-
fore, that, as Captain Osborn suggests, Greenland extends
much farther north than it has yet been traced, we
still have evidence that it is after all an island, around
which the Atlantic sends its tides to Kennedy Channel.

There is probably open sea communication between
the Pacific and the North Pole. Supposing *both* views
to be correct, a remarkable result follows. It would
be possible, after making the North Pole, by following
the valley of the Atlantic northwards, to travel south-
wards to Behring's Straits, and thence to China. We
should have, not a north-west passage, but a 'north-

and-south ' passage, *very much shorter* than the long-sought north-west route. If the reader will be at the pains to take a terrestrial globe, and, having placed one end of a string near London, to extend the string *tightly* over the North Pole and so round to the Pacific, he will find that the string passes over Behring's Straits. The north-westerly route is far removed from the string, *which, being stretched, necessarily indicates the shortest path between any two points on its length.* In fact, the ' great-circle route ' from London to Behring's Straits passes close to the North Pole.

But it is not a little singular that, in going out of their way to follow the dangerous channels to the north of the American continent, sailors also left the path which promises the mildest climate. The North Pole is, in all probability, far from being the pole of cold whether in winter or in summer, or taking the mean of the whole year. It is well known that there are two poles of winter cold, one lying at Yakutsk, in Siberia, the other lying to the north of America, not far from Wellington Channel. The poles of summer cold have not yet been determined. But from the position of the coldest summer isotherm yet traced, there would seem to be three poles of summer cold—one in the northern parts of Greenland, the other near Novaia Zemlia, the third near Behring's Straits. But, of course the greatest cold in summer is a very different matter from the greatest cold in winter; and the position of the summer poles of cold is proportionately less important to our inquiry. The poles of mean

annual cold seem to lie one to the north of Siberia, the other to the north of America, the true pole lying almost midway between them.

It would seem, therefore, that our sailors, in leaving the shortest path leading from England to Behring's Straits, and following a series of dangerous and often ice-bound straits, have actually gone to that very region which contains the western poles of winter cold, of summer cold, and of mean annual cold. It would have been as reasonable, it now appears, to have attempted to follow the northern shores of Asia as the northern shores of America. Nay, *the former course is rather the shorter of the two.*

I have not dealt at length with the evidence in favour of the existence of a wide open sea around the North Pole. The subject, indeed, is a very extensive one. But I may mention one of the most important arguments.

Every year there is thrust out from Davis's Strait and Baffin's Bay a tongue of ice, one thousand miles or more in length. The ice near the shore gives way first, and as spring advances the middle ice (which had drifted in early winter from more northern latitudes) is floated down into the Atlantic. So certainly is this phenomenon reproduced year after year that whalers who wish to cross Baffin's Bay always make for northern latitudes, confident that they will be able to get round the northern extremity of the ice-tongue as it floats out. The *Resolute* was abandoned by officers and crew in lat. 74° 40′, long. 101° 20′, and discovered

afloat off Cape Mercy in lat. 55°. The *Fox* was frozen
up in August 1857, and remained so for 242 days,
during which time she drifted 1,194 miles to the south-
ward. The *Advance*, after having been carried as far
north as 75° 25′, was drifted southwards for 1,000 miles,
and liberated in lat. 65° 30′. These vessels were
drifted *with* the ice, not *through* it ; for De Haven, of
the *Advance*, reports that when he was liberated he had
the same 'hummocks,' the same snow-drifts, and the
same icy landscape which set out with him. Now it is
abundantly evident that we cannot account for such
remarkable drifts as these, without assuming that, far
to the northward, there exist large open tracts of water,
whence the currents which carry down these ice-rafts
are supplied.

The journey to the North Pole, by whatever route, must
necessarily be one accompanied by a certain amount of
danger. It will also tax, not lightly, the energies of
those engaged upon it. But considerations such as
these ought not to deter Englishmen from attempting to
'hold their own' in Arctic seas. To the *cui bono* which
is the ever-ready question of narrow-minded men, I
answer that *if* these regions were known, the question
might perhaps be proved to be an idle one. It is just
because we know nothing of them that we are unable to
say exactly what benefits might spring from exploration.
I can point, however, to one large mercantile class—
our whale-fishers—who might reap material advantage
from the information which would doubtless be acquired
in a North Polar expedition. But I shall conclude by

quoting one who has adopted a nobler view of the sub-
ject than that founded merely on the prospects of
tangible profit.

'Researches,' says Captain Maury, 'have been carried
on from the bottom of the deepest pit to the top of the
highest mountain, but these have not satisfied. Voyages
of discovery, with their fascinations and their charms,
have led many a noble champion of human progress
both into the torrid and frigid zones; and notwith-
standing the hardships, sufferings, and disasters to
which many northern parties have found themselves
exposed, seafaring men, as science has advanced, have
looked with deeper and deeper longings towards the
mystic circles of the Polar regions. There icebergs
are framed and glaciers launched; there the tides have
their cradle, the whales their nursery; there the winds
complete their circuits, and the currents of the sea
their round in the wonderful system of oceanic circula-
tion; there the aurora is lighted up and the trembling
needle brought to rest; and there, too, in the mazes of
that mystic circle, terrestrial forces of occult power
and of vast influence upon the well-being of man are
continually at play. Within the Arctic Circle is the
pole of the winds, and the poles of cold; the pole
of the earth and of the magnet. It is a circle of
mysteries; and the desire to enter it, to explore its
untrodden wastes and secret chambers, and to study
its physical aspects, has grown into a longing. Noble
daring has made Arctic ice and snow-clad seas classic
ground. It is no feverish excitement nor vain ambition

that leads man there. It is a higher feeling, a holier motive—a desire to look into the works of creation, to comprehend the economy of our planet, and to grow wiser and better by the knowledge.'

RAIN.

THERE are, perhaps, few natural phenomena which appear less indicative, at first sight, of the operation of nature's giant forces than the downfall of rain. Even the heaviest showers—at least of those we are familiar with in England—are not phenomena which suggest an impression of power. Yet the forces actually called into action before rain can fall, are among the most gigantic experienced on our earth. Compared with them, *terrestrial* gravitation is more feeble than is the puniest infant compared with an army of giants. Let us look into the matter a little closely, and we shall see that this is so.

It is a common occurrence for rain to fall over an area of 100 square miles to a depth of one inch in twenty-four hours. Now, what is the expenditure of power of which such a phenomenon is the equivalent? The downfall is, so to speak, the loosening of the spring, but how much force was expended in winding up the spring? The evaporation from the sea or from moist soils of the quantity of water precipitated, is not the whole of

the work to be estimated, since the vapour has to be raised to the higher regions of the air, and to be wafted by the winds—themselves the representatives of giant forces—to the district over which the moisture is discharged in rain. But let us take this evaporation only, and estimate its real force-equivalent. It may be shown by a calculation founded on Mr. Joule's experiments, that to evaporate a quantity of water sufficient to cover an area of 100 miles to the depth of one inch, would require as much heat as is produced by the combustion of *half a million tons of coals*; and further, that the amount of force of which such a consumption of heat is the equivalent, corresponds to that which would be required to raise a weight of upwards of one thousand millions of tons to a height of one mile! I will run briefly through the calculation by which this last result is deduced from the well-known result of Joule's experiments that to raise one pound of water one degree Fahrenheit, requires a quantity of heat sufficient to raise one pound to a height of 772 feet; and the further experimental fact, that to raise a pound of water from the liquid to the vaporous state, requires 967 times as much heat as is required to raise the same pound one degree Fahrenheit in heat.

The amount of water required to cover one hundred square miles to a depth of one inch is, in volume—

$$1{,}760 \times 1{,}760 \times 3 \times 3 \times 100 \div 12$$

cubic feet, and as one cubic foot of water weighs 1,000 oz., or nearly 63 pounds, we have in weight—

$$1{,}760 \times 1{,}760 \times 3 \times 3 \times 8\tfrac{1}{2} \times 62\tfrac{1}{2} \text{ pounds,}$$

and to raise this weight of water 1° F., would require as much heat as would suffice to raise to a height of *one mile* a weight of—

$$1{,}760 \times 3 \times 8\tfrac{1}{2} \times 62\tfrac{1}{2} \times 772 \text{ pounds;}$$

while to vaporize the same weight of water would require 967 times as much heat. Thus we obtain a force sufficient to raise a weight of—

$$1{,}760 \times 3 \times 17 \times 135 \times 193 \times 967 \text{ pounds,}$$

(that is, nearly 1,020,000,000 tons), to the height of one mile.

Such is the amount of force, whose effects are exhibited in a day's steady downpour over a region of 100 square miles—for instance, over about one-third of Middlesex.

The same amount of water falling in the form of snow, would represent a yet greater expenditure of force. 'I have seen,' says Tyndall, 'the wild stone-avalanches of the Alps, which smoke and thunder down the declivities, with a vehemence almost sufficient to stun the observer. I have also seen snow-flakes descending so softly as not to hurt the fragile spangles of which they were composed; yet to produce, from aqueous vapour, a quantity which a child could carry, of that tender material, demands an exertion of energy competent to gather up the shattered blocks of the largest stone-avalanche I have ever seen, and pitch them to twice the height from which they fell.'

But it is when we come to estimate the fall of
rain as a terrestrial phenomenon—as a process con-
tinually going on over large regions of the earth's
surface, as a process in which energies exhibited over
one region are expended, frequently, over regions
thousands of miles away—that we see the full signifi-
cance of the drop of rain. We can well understand
how it is that ' the clouds drop fatness on the earth,'
when we estimate the powers expended in their genesis.
All the coal which could be raised by man from the
earth in thousands of years, would not give out heat
enough to produce by evaporation the earth's rain-
supply for one single year ! The sun—whose influence
is often contrasted with that of the rain-shower—is the
agent in producing that shower as well as in pouring
out his direct heat on the soil with such apparently
contrasted effect.

The actual process of the production of rain has
not yet been completely explained. We are, in fact,
doubtful as to the true nature of clouds, fogs, and
mist, and, therefore, it is intelligible that some
difficulty should surround the explanation of a pheno-
menon of which these meteors are, so to speak, the
parents.

It is generally supposed that clouds consist mainly
of hollow *vesicles* of water, and not of minute drops.
Yet meteorologists are far from being agreed on this
point.   On the one hand we have the evidence of De
Saussure and Kratzenstein, who actually saw, or sup-
posed they saw, the constituent vesicles of clouds and

fogs. De Saussure, indeed, tells us how we may see
the vesicles for ourselves. If a cup of coffee, or of
water tinctured with Indian ink be placed in the
sun, minute vesicles of various thickness will be seen
to ascend from the surface of the liquid. He adds that
those vesicles which rise differ so much in appearance
from those which fall, that it is impossible to doubt
that the former are hollow. Kämtz, also, made
measurements of the vesicles of fogs in Central Ger-
many and in Switzerland; and in his valuable work on
Meteorology, gives a table and a figure, showing the
law according to which the dimensions of the vesicles
vary in the course of the year.

Despite this evidence, Sir John Herschel holds a
contrary opinion. He points out that the observations
of De Saussure and Kratzenstein may be readily re-
ferred to the effect of optical illusion. The strongest
argument put forward by Kratzenstein is founded on the
fact that rainbows are never formed on clouds or fogs, as
they would be (according to the undulatory theory of
light) if these meteors were composed of globules of
water. Sir John Herschel, a higher authority on
optical questions than either De Saussure or Kratzen-
stein, is of opinion, on the contrary, that it is possible
a re-examination of the very difficult point in ques-
tion would give a different account than that usually
accepted.

Herschel points out the difficulty of understanding
in what manner the condensation of true vapour should
result in the formation of a hollow vesicle. Tyndall

points out, on the other hand, a difficulty depending on the state into which water particles at high elevations sometimes pass. 'It is certain,' he says, 'that they possess, on or after precipitation, the power of building themselves into crystalline forms; they thus bring forces into play which we have hitherto been accustomed to regard as molecular, and which could not be ascribed to the aggregates necessary to form vesicles.'

In whatever state the particles of a cloud really exist, it is certain that the fall of rain depends on a process of increased condensation. The causes producing such condensation have been thus summed up by Professor Nichol :—

(1). The cooling of clouds through the effect of radiation from them;

(2). The mingling of vapours at different temperatures—a mingling effected by the agency of the winds;

(3). The rising of vapours towards colder strata of the atmosphere;

(4). The increase of atmospheric density or pressure;

And (5). The accumulation and impinging of masses of vapour against some obstacle.

Singularly enough he omits the most important of all known agencies in the production of rain, viz. :—

(6). The transfer from the equator towards the

poles of large masses of moisture-laden air by means of the upper S.W., or counter trade-winds.

I must note also that cause (4) is more than doubtful. Tyndall has shown that rarefaction is an efficient agent in producing the precipitation of vapour. By increase of pressure a larger quantity of moisture is, indeed, compressed within any given space; but, on the other hand, there is an increase of heat within the space which more than counterbalances the former in effect. 'The heat developed,' says Tyndall, speaking of an experiment illustrating the effects of increased pressure, 'is more than sufficient to preserve it' (the moisture added to a given space) 'in the state of vapour.'

It will be seen at once from the above imperfect enumeration of causes affecting the production of rain, that the phenomenon is no simple one. When we add the variety of circumstances affecting the action of different causes—as the latitude of the place; the elevation above the sea-level; the proximity of the sea; the laws affecting the seasonal variations at the place; the prevailing winds; the configuration of the surrounding surface, it will become evident that meteorologists may well be perplexed by the very complex set of agencies acting in the production of rain; and so fail—as they have hitherto done—in interpreting any save the most general laws influencing the phenomenon.

Some of these general laws I now proceed to consider.

In the first place, it may be accepted as generally true that the amount of moisture present in the atmosphere is greatest near the equator, and diminishes towards the poles. With the sun's change of declination the zone of greatest moisture passes to the north or to the south of the equator, *following* the sun. The mean region, it is to be noted, is not absolutely coincident with the equator, but some four or five degrees north of that circle. It is easily intelligible that the hottest regions should be, *cœteris paribus*, those over which the amount of moisture present in the atmosphere is greatest, since the heat vaporises the water over these regions. It may not seem, at first sight, quite so obvious that the same regions of greatest heat should also be those in which the rainfall should be in general heaviest. For it might appear that the same heat which produced the evaporation would maintain the water in a state of vapour. The fact, however, that aqueous vapour is lighter than air, operates to produce ascending currents over the region of evaporation, currents strengthened by the expansive effects of the heat. Accordingly, the vapour rises rapidly, and when it has thus risen, many circumstances operate to produce precipitation. First, the upper regions are rarer; secondly, they are colder; thirdly, radiation of heat takes place rapidly from the upper surface of clouds, brought here, as Tyndall expresses it, into the presence of pure space (*dry air* having scarcely any appreciable effect in checking radiation). The result is, that the uplifting of clouds under the sun's influence is followed regularly

over the equatorial regions by the precipitation of heavy rain-showers. And *cæteris paribus,* the fall of rain decreases with distance from the equator of heat, though not so regularly as the amount of moisture decreases.

The next great law which presents itself to our consideration is this, that winds blowing towards the equator are, in general, dry winds, and winds blowing from the equator, rainy. This law is the direct consequence of the former, but it is necessary, for several reasons, to present it as a separate law. There is an erroneous method of accounting for this law which is very commonly met with in works on meteorology. It is argued, that as winds blowing towards the equator are carrying masses of air from colder to warmer regions, they are necessarily dry winds, since, if the air is saturated, or nearly so, at starting, it cannot be saturated when it has become warmer. And *vice versâ,* winds blowing towards the poles are carrying masses of air to colder regions. The air accordingly grows colder, and if not far from being saturated at starting, cannot fail to become unable to keep its whole burden of moisture in the state of vapour, and must accordingly precipitate a portion as rain. This explanation is insufficient. It would, indeed, be just as reasonable to reverse the argument thus : a wind blowing towards the equator must bring rain; for as it brings cold air into warm regions, if the air in these regions is nearly saturated, the introduction of cold air must lead to the precipitation of a part of the moisture, and *vice versâ,*

a wind blowing towards the poles must be a dry, because
it is a heat-bearing wind. The simple explanation of
the law is, that winds blowing towards the equator are
dry, because they are blowing from regions over which
moisture is less, to regions over which moisture is more
abundant, and *vice versâ.*   Of course we must superadd
to this the facts mentioned above, because a moist wind
blowing towards a heated region would not bring rain
with it, and a comparatively dry wind, blowing towards
a cold region, might bring rain.   But it must not be
forgotten that the main question to be considered is
the relative moistness of the transported masses of air.

We meet with corresponding laws affecting the
rain-producing powers of winds travelling over conti-
nents and oceans. A wind blowing over an ocean
towards a continent brings rain to the continent, unless
the heat over the latter exceeds slightly, or at the least,
does not fall short of, the heat over the neighbouring
ocean.   Such a wind is certain to bring rain to an
elevated continental region not protected by a moun-
tain barrier yet more elevated.   On the other hand, a
wind blowing over a continent towards the ocean in
general brings no rain.

Lakes, marshes, and rivers, act in a small way a
similar part towards the adjoining lands as oceans
towards neighbouring continents.

There are circumstances also to be considered as
affecting the rainfall in a different manner, viz., not by
supplying a greater or less amount of moisture to the
atmosphere, but by affecting the power of the atmo-

sphere to keep the moisture it supports in the
vaporous state. Such are the *contour* and *elevation*
of a country, the nature of its soil, the quantity of
forest land, or, wanting this, the relative abundance
or paucity of trees, and so on.

A moist and warm current of air impinging on a
mountain range, or even on any well-defined rising
slope, so as to be carried with sufficient suddenness to
colder and rarer regions, is compelled to part with a
large portion of its moisture in the form of rain; and
conversely a wind which has passed over a mountain
range or an elevated plateau, and descends to a lower
region, appears as a dry wind, unless that region is one
over which a continual process of evaporation sufficient
to maintain the air nearly in a state of saturation is
going on. In this latter case the effects of the de-
scending wind will vary with circumstances. It will in
general appear as a dry wind, but may produce local
showers, since it may act, through the sudden addition
of cold air, the part of a condenser.

Forests are great generators of rain. This is mainly
due to the peculiar radiative power of trees and vege-
tables. The soil, where it is covered with vegetation,
receives no heat directly from the sun, and but little
through contact with the heated air. It may seem
like a confusion of cause and effect to speak of vegeta-
tion-covered countries as rain-generators, since abundant
rain is so important a requisite for the abundant
growth of vegetables. This is, however, a case in
which cause and effect are interchangeable. Rain en-

courages vegetation, and vegetation in turn aids in producing a state of the superincumbent atmosphere which encourages the precipitation of rain. The result is that, apart from external agencies, regions covered with abundant vegetation, and especially with high trees, present year after year, and century after century, a ranker and yet ranker luxuriance of vegetable growth.

On the contrary, arid regions prevent, by their very aridity, and consequently by the intense heat of the soil and superincumbent air, the downfall of the showers which would nourish vegetation. The result is, that even when the soil itself is favourable, it is exceedingly difficult to convert an arid into a vegetation-covered district, the want of moisture being destructive to trees planted in such soils with the object of encouraging rain-fall. The process of change must be a gradual one. On the other hand, the improvement of a region over which rain falls too heavily through overabundant vegetation is a comparatively simple process, a judicious system of clearing invariably leading to the desired result.

The influence of the seasons remains yet to be mentioned among the circumstances affecting the distribution of rain over the earth's surface. The influence of the seasons is different in different zones of the earth's surface. Under the tropics the laws affecting the fall of rain are much more regular than elsewhere. On the ocean we have clear skies where the trade-winds are blowing steadily, and heavy rain falls by day over the

intermediate zone of calms; but on the land we have regular dry and wet seasons within the tropics. There is, properly speaking, no winter or summer; but applying these terms to the periods at which winter or summer prevails in the temperate zones of either hemisphere, we may say that the sky is serene in the winter, becomes moist in spring, and the rainy season sets in when the sun is near the zenith. Where there is a considerable interval between the sun's passages of the zenith, as in places not very far from the equator, there are two wet seasons, both occurring in summer. In countries in which monsoons prevail, however, the alternation of dry and wet seasons depends on the winds. When the south-west monsoon is blowing over India, for instance, there is no rain on the east coast, but abundant rain on the west coast. During the north-east monsoon these conditions are reversed. A little consideration will show that all the above-mentioned seasonal variations within the tropics depend on general laws already stated.

Beyond the tropics there is less regularity. The fall of rain depends on the prevalence of certain winds which bring moisture with them, and, these winds not blowing with any regularity, the rainfall is similarly irregular. In countries close to the tropics, there is a noteworthy dryness in summer; for this reason clearly, that in summer the trades blow over these regions, and bring with them 'trade-wind weather.' Further north, however, though there may be a tendency to the prevalence of north-easterly winds in summer, this ten-

dency is not so marked as to produce a considerable
defect of rain in the summer as compared with the
winter months.[1]

In England we have one cause affecting the rain-
fall which is worthy of special notice. I refer to the
Gulf Stream. The air above this warm stream is not
only warmer than the surrounding air, but is heavily
laden with moisture. When the western and south-
western winds loaded with the vapour of water begin
to blow over England, they precipitate their moisture
in rain, as they encounter the colder air, over the land ;
but the manner in which this happens is variable with
the seasons, for in the winter months the moisture-laden
winds blow lower, and therefore precipitate their
vapour earlier ; whereas in summer the clouds range
higher, and therefore travel farther inland before they
fall in rain. The same effects are observable in the
Scandinavian peninsula, Norway receiving more rain in
winter than in summer ; while Sweden, on the eastern
side of the Dovrefields, receives more rain in summer
than in winter.

Such are some of the general laws which affect the
downfall of rain in various countries and at different
seasons. There is one circumstance involving the
action of a yet grander law—about which, however,
considerable uncertainty still exists. I refer to the

[1] So far as my own observations extend, I should say that the two
features of our climate which may be most certainly depended on—
which, be it noted, is not saying much—are, heavy rains in July, gene-
rally in the last fortnight, and serene weather during the second week of
November.

difference observable between the northern and the southern hemispheres. It has been already noted that the mean position of the medial zone of calms and heavy diurnal rainfalls lies some 4° or 5° to the north of the equator. The total annual downfall of rain north of this medial line is slightly greater (so far as our present information extends), than the downfall south of the medial line. And, therefore, since the area of the northern region is less than the area of the southern, it is clear that the annual downfall over any northern zone is, in general, considerably heavier than the downfall over the corresponding southern zone. Now, if we remember that the amount of aqueous vapour raised by evaporation over the southern or watery hemisphere must necessarily be much greater than the amount raised over the northern hemisphere, this result will appear a remarkable one. One would expect to find a difference—and a very marked difference—between the two hemispheres; but instead of the excess of rainfall being in favour of the northern hemisphere, one would expect it to have been in favour of the southern.

If we assume with Maury that the north-easterly and south-easterly trade winds which meet near the equator merge, respectively, into the north-westerly and south-westerly counter-trades; that is, that they cross over to the opposite hemisphere to that in which they were generated, the difficulty seems to vanish. For in this case, the downfall over the northern hemisphere is due to evaporation over the southern hemisphere, and *vice versâ.* Maury adduces other arguments in favour

s

of his theory of an intercrossing of this sort. Sir
John Herschel, however, will not listen to Maury's
views. He 'declines adopting the doctrine recently
propounded of a systematic crossing of the south-east
and north-east trades at the medial line. In so doing,'
he is 'in no way disturbed by the phenomenon of
infusorial dust of South American origin which occa-
sionally falls on the north-east of Africa,' and so on.   I
must confess that the balance of evidence seems to me
to lie on Maury's side in this instance.

It may be asked, however, whether there is any
occasion to adopt either view as a systematic account of
the laws affecting the trades and counter-trades.   May
not Maury and Herschel be like the two knights who
saw opposite sides of the same shield, and who—both
right and both wrong—were persuaded, one that the
shield was silvern, the other that it was golden?

If we remember that the medial line marks a zone
of calm towards which, from either hemisphere, im-
mense masses of moisture-laden air are continually
being swept in, why should we arbitrarily assign to the
masses of air passing away above from this calm zone,
such a law of motion that every particle of air which
has originally come from the northern hemisphere shall
take one course, and every particle which has come
from the southern shall take an opposite one.   It
appears to me, on the contrary, that an intermingling
(in masses, it may be, but still complete), must take
place above, and result in an almost indifferent diffusion
of the vapour-laden air northwards and southwards

with the returning counter-trades. The fact that the northern trades have a southerly motion as they enter the calm zone (passing here upwards), and *vice versâ,* may lead to a slight preponderance of air (originally) from the northern hemisphere in the north-westerly counter-trade, and *vice versâ,* but by no means (I should think) to anything approaching the systematic intercrossing imagined by Maury. On the other hand, the preponderance might lie the other way, owing to the effects of collision between the northern and southern trades—but without leading to the systematic return of northern air to the northern temperate zone, and of southern air to the southern temperate zone, conceived to take place by Sir J. Herschel.

One of the most remarkable results of observations made upon rain, has been the discovery that the amount of fall at any place diminishes largely as the rain-gauge is raised above the level of the ground. It is not very easy to explain this remarkable fact. The explanation offered by Kämtz is, that a falling drop carries with it the temperature of the upper regions of air, and condenses on its surface the aqueous vapour present throughout the lower strata of the atmosphere, as a decanter of cold water does when brought into a room. And of this explanation Professor Nichol remarks, that ' it is not an hypothesis but a rigorous deduction, giving an account of all the facts as yet ascertained in connection with this subject.' But unfortunately, the explanation, though it undoubtedly presents a *vera causa,* will not bear the test of 'quantitative analysis.'

Sir John Herschel has gone through the simple calcu-
lation required to overthrow the theory, and points out,
that if we allow to the cause the full value it can
possibly have (a value far exceeding that which can
probably be attributed to it) we obtain an effect only
one-seventeenth part of what is wanted to account for
the phenomenon. . Sir John points out also that
obliquity of fall cannot possibly affect the observed
amount of rainfall, and he offers no hypothesis in
explanation of the phenomenon, and remarks in con-
clusion, that 'visible cloud rests on the soil at low
altitudes above the sea-level but rarely ; and from
such clouds alone would it seem possible that so large
an accession of rain could arise.' He refers, however,
in a note to a paper read by Mr. Baxendell to the
Literary and Philosophical Society of Manchester on
this subject, in which it is inferred that the only way
of accounting for the phenomenon lies in the admission
of the existence of water 'not in the state of true
vapour,' but already deprived of its latent caloric,
though not affecting the transparency of the air, so.
that 'a shallow stratum of the lower and comparatively
clear atmosphere' may 'supply as much rain as a
densely-clouded and much deeper stratum in the
higher regions.' Baxendell mentions also the interest-
ing fact, that the drops of water which drip from the
upper part of the shaft increase to an extraordinary
size in the descent to the bottom.

It appears to me that the well-known phenomenon

of rain falling from a clear sky—a rain termed by the French *serein*—has a suggestive bearing on the peculiarity we have been considering. It proves that water may exist, even in drops, in the atmosphere, without appreciably affecting its transparency. And though it may be an uncommon thing for rain to fall without appearing first in the upper regions of air—in the form of cloud, yet it by no means follows that *during a shower* rain might not be falling from the lower as well as from the upper air-strata, without the transparency of the lower strata being much or at all affected. I have noticed, always, that if the eye be directed steadily at the drops of heavily-falling rain, there will be seen flitting, as it were, among them minute specks, which are seen on a closer observation to be small particles of water. Now, it does not appear to me likely that these, or most of them, are produced by the collision of the falling drops—for the paths of two neighbouring drops must be parallel, since the drops are subjected to precisely the same set of influences.

I believe the phenomenon to be one worthy of more careful notice than it has received—in fact, I am not aware that it has been noticed at all. The motions of the particles are themselves interesting—seeming almost as independent of gravitation, wind-currents, or the like, as the motion of a flight of insects would be. It is hardly necessary to observe that if these particles show that rain is being generated in the lower as well as the upper strata of the air, all difficulty in explaining

the results of Professor Phillips's observations, vanishes
at once.

DANGER FROM LIGHTNING.

WHEN we hear that so many persons are struck by
lightning in the course of a year, we are apt to regard
the danger from lightning as greater than it really is ;
and thus the feelings of awe and terror which many ex-
perience during the progress of a thunderstorm are too
often increased.   In reality, the danger to which we are
exposed during such storms is far from great, more
especially in towns.   It is well that this should be
known, because the effects produced on persons of
nervous temperament by the vivid flashes of lightning
and the resounding peals of thunder, are sufficiently
painful, without that additional and even more dis-
tressing terror which the apprehension of real danger
commonly produces.   Instances have been known of
death being occasioned by the dread which a thunder-
storm has excited, when the seat of danger was in reality
several miles away.

There are, however, persons, not otherwise wanting
in courage, who experience an oppressive sense of terror
—apart from the fear of danger—when electrical
phenomena are in progress.   The Emperor Augustus
used to suffer the most distressing emotions when a

thunderstorm was in progress ; and he was in the habit
of retiring to a low vaulted chamber underground,
under the mistaken notion that lightning never pene-
trates far below the earth's surface.   Major Vokes, the
Irish police-officer—a man whose daring was proverbial
—used to be prostrated by terror during a thunder-
storm.   We cannot doubt that, in these instances,
nervous effects are produced which are wholly distinct
from the fear engendered by the simple consciousness
of danger.

We have said that the danger is small when a
thunderstorm is in progress.   If we consider the num-
ber of persons exposed during a year, in England,
to the effects of lightning-storms raging in their
immediate neighbourhood, and compare with that
number the small number of recorded deaths, we shall
see that the *probability* of being struck by lightning
is very small indeed.   The danger we are exposed to
in travelling along the most carefully regulated rail-
way, is many times greater than that to which, under
ordinary circumstances, we are exposed when a thunder-
storm is raging around us.   Yet, in cases of this sort,
men do not reason according to the doctrine of chances
—nor, indeed, is it desirable that they should.   There
are measures of precaution which, small though the
danger may be, it is well to adopt.   In a railway
carriage, it would be foolish to let the mind dwell
upon the danger to which we are in reality exposed,
since we can do nothing towards diminishing it.   But it
would be as unreasonable to neglect precautions in the

presence of a heavy thunderstorm, merely because the
danger of being struck is small, as it would be to
neglect the rules which regulate powder-stores, merely
because the instances in which fires have been caused
by carrying cigar-lights in the coat-pocket, or by
wearing iron on the sole of the boot, are few and far
between.

We have mentioned one precautionary measure
adopted by the ancients. The notion that lightning
does not penetrate the earth to any considerable depth,
was in ancient times a widespread one. It is still pre-
valent in China and Japan. The emperors of Japan,
according to Kæmpfer, retire during thunderstorms
into a grotto, over which a cistern of water has been
placed. The water may be designed to extinguish fire
produced by the lightning; but more probably it is
intended as an additional protection from electrical
effects. Water is so excellent a conductor of electricity,
that, under certain circumstances, a sheet of water
affords almost complete protection to whatever may be
below; but this does not prevent fish from being killed
by lightning, as Arago has pointed out. In the year
1670, lightning fell on the lake of Zirknitz, and killed
all the fish in it, so that the inhabitants of the neigh-
bourhood were enabled to fill twenty-eight carts with
the dead fish found floating on the surface of the lake.
That mere depth is no protection is well shown by the
fact of those singular vitreous tubes called fulgurites,
which are known to be caused by the action of light-
ning, often penetrating the ground to a depth of thirty

or forty feet. And instances have been known in which lightning has ascended from the ground to the storm-cloud, instead of following the reverse course. From what depth these ascending lightnings spring, it is impossible to say.

Still, we can scarcely doubt that a place underground, or near the ground, is somewhat safer than a place several storeys above the ground floor.

Another remarkable opinion of the ancients was the belief that the skins of seals or of snakes afford protection against lightning. The Emperor Augustus, before mentioned, used to wear seal-skin dresses, under the impression that he derived safety from them. Seal-skin tents were also used by the Romans as a refuge for timid persons during severe thunderstorms. In the Cevennes, Arago tells us, the shepherds are still in the habit of collecting the cast-off skins of snakes. They twist them round their hats, under the belief that they thereby secure themselves against the effects of lightning.

Whether there is any real ground for this belief in the protecting effects due to seal-skins and snake-skins is not known; but there can be no doubt that the material and colour of clothing are not without their importance. When the church of Châteauneuf-les-Moutiers was struck by lightning during divine service, two of the officiating priests were severely injured, while a third escaped—who alone wore vestments ornamented with silk. In the same explosion, nine persons were killed, and upwards of eighty injured. But it is noteworthy that

several dogs were present in the church, *all of which were killed.* It has also been observed that dark-coloured animals are more liable to be struck (other circumstances being the same) than the light-coloured. Nay, more; dappled and piebald animals have been struck; and it has been noticed, that after the stroke, the hair on the lighter parts has come off at the slightest touch, while the hair on the darker parts has not been affected at all. It seems probable, therefore, that silk and felt clothing, and thick black cloth, afford a sort of protection, though not a very trust-worthy one, to those who wear them.

The notion has long been prevalent that metallic articles should not be worn during a thunderstorm. There can be no doubt that large metallic masses, on or near the person, attract danger. Arago cites a very noteworthy instance of this. On July 21, 1819, while a thunderstorm was in progress, there were assembled twenty prisoners in the great hall of Biberach Jail. Amongst them stood their chief, who had been con-demned to death, and was chained by the waist. A heavy stroke of lightning fell on the prison, and the chief was killed, while his companions escaped.

It is not quite so clear that small metallic articles are sources of danger. The fact, that when persons have been struck, the metallic portions of their attire have been in every case affected by the lightning, affords only a presumption on this point, since it does not follow that these metallic articles have actually attracted the lightning-stroke. Instances in which a

metallic object has been struck, while the wearer has escaped, are more to the point, though some will be apt to recognise here a protecting agency rather than the reverse. It is related by Kundmann that a stroke of lightning once struck and *fused* a brass bodkin worn by a young girl to fasten her hair, and that she was not even burned. A lady (Arago tells us) had a bracelet fused from her wrist without suffering any injury. And we frequently see in the newspapers accounts of similar escapes. If it is conceded that in these instances the metal has attracted the lightning, it will, of course, be abundantly clear that it is preferable to remove from the person all metallic objects, such as watches, chains, bracelets, and rings, when a thunderstorm is in progress. If, on the other hand, it is thought that the lightning, which would in any case have fallen towards a person, has been attracted by the metal he has worn, so as to leave him uninjured, the contrary view must be adopted. Mr. Brydone considers that a thin chain attached in the manner of a conductor to some metallic article of attire, would serve in this way as an efficient protection. Our own opinion is, that, in general, metallic articles belonging to the attire are not likely to have any noteworthy influence, but that such influence as they do exert is unfavourable to safety. We may agree with Arago, however, that ' it is hardly worth while to regard the amount of increased danger occasioned by a watch, a buckle, a chain, pieces of money, wires, pins, or other pieces of metal employed in men's or women's apparel.'

Franklin recommends persons who are in houses not protected by lightning-conductors, to avoid the neighbourhood of the fire-place; for the soot within the chimney forms a good conductor of electricity, and lightning has frequently been known to enter a house by the chimney. He also recommends that we should avoid metals, gildings, and mirrors. The safest place, he tells us, is in the middle of a room, unless a chandelier be suspended there.

His next rule is not a very useful one. He recommends that we should avoid contact with the walls or the floor, and points out how this is to be done. We may place ourselves in a hammock suspended by silken cords; or, in the not unlikely absence of such a hammock, we should place ourselves on glass or pitch. Failing these, we may adopt the plan of placing ourselves on several mattresses heaped up in the centre of the room. We do not think that such precautions as these are likely to be commonly adopted during a thunderstorm, nor does it seem necessary or desirable that they should be. We have not even the assurance that they greatly diminish the danger. A stroke of lightning which fell on the barracks of St. Maurice at Lille, in 1838, pierced the mattresses of two beds through and through.

That glass is a protection from lightning is an opinion which has been, and perhaps still is, very prevalent; yet there have been many instances tending to prove the contrary. In September 1780, Mr. Adair was struck to the ground by lightning, which killed two

servants who were standing near him. The glass of
the window had not only offered no effective resistance
to the lightning, but had been completely pulverised by
it, the framework of the window remaining uninjured.
Again, in September 1772, lightning pierced through
a pane of glass in a window on the ground floor of a
house in Padua, 'making a hole as round as if drilled
with an auger.'

It seems to have been established that if a thunder-
storm is in progress, a building is in more danger of
being struck when many persons are crowded within it,
than when few are present. This points to the danger
of the course sometimes followed by the inmates of a
house during a thunderstorm. They appear to think
that there is safety in society, and crowd into one or
two rooms, that they may try, by conversation and
mutual encouragement, to shake off the feeling of
danger which oppresses them. They are in reality
adding, and that sensibly, to any danger there may be.
'There is,' says Arago, 'a source of danger where large
assemblages of men or animals are present, in the
ascending currents of vapour caused by their perspira-
tion.' Like water, moist air is a good conductor of
electricity, and lightning is attracted in the same way
—though not, of course, to the same extent, by an
ascending column of vapour, as by a regular lightning-
conductor. It is on this account, probably, that flocks
of sheep are so frequently struck, and so many of them
killed by a single stroke. Barns containing grain
which has been housed before it is quite dry are more

commonly struck by lightning than other buildings, the ascending column of moist air being probably the attracting cause in this case, as in the former. When we are overtaken by a thunderstorm in the open air, precaution is more necessary than within a house. It is well to know, especially when no shelter is near, what is the most prudent course to adopt.

It has been stated that there is danger in running against the wind during a thunderstorm, and that it is better to walk with than against the wind. One should even, it is said, if the wind is very high, run with the wind. The *rationale* of these rules seems to be this: a current of air is produced when we run against the wind, the air on the side turned *from* the wind being rarer than the surrounding air. A man so running ' leaves a space behind him in which the air is, comparatively speaking, rarefied '! Lightning would be more likely to seek such a space for its track than a region in which the air is more dense. An instance is recorded in which, during a gale, lightning actually left a conductor which passed from the mast of a ship to her windward side, in order to traverse the space of rarefied air on the ship's larboard side !

It is quite certain that trees are very likely to be struck by lightning, and, therefore, that it is an exceedingly dangerous thing to stand under trees in a storm. No consideration of shelter should induce any one to adopt so dangerous a course. The danger, in fact, is very much greater when heavy rain is falling, since the tree, loaded with moisture, becomes an

efficient lightning-conductor. For similar reasons, it
is dangerous to seek the shelter of a lofty building
(not protected by a lightning-conductor) in a thunder-
storm. One of the most terrible catastrophes known
in the history of thunderstorms occurred to a crowd of
persons who stood in the porch of a village church
waiting till a thunder-shower should have passed away.

In the open air, when a heavy thunderstorm is pro-
gressing, and no shelter near, the best course is to
place one's self at a moderate distance from some tall
trees. Franklin considered a distance of about fifteen
or twenty feet the best. Henley also considered five
or six yards a suitable distance in the case of a single
tree. But when the tree is lofty, a somewhat greater
distance is preferable.

The reader need hardly be reminded, perhaps, that
the necessity for taking these precautions only exists
when the storm is really raging close at hand. When
the interval which elapses between the lightning-flash
and the thunder-peal is such as to show that the storm
is in reality many miles away, it is altogether
unnecessary to take precautions of any sort, however
brilliant the flash may be, or however loud the peal.
It must be noticed, however, that a storm often travels
very rapidly. If the interval of time between the
lightning and the thunder is observed to diminish
markedly, so that the storm is found to be rapidly
approaching the observer's station, the same pre-
cautions should at once be taken as though the storm
were raging immediately around him. So soon as the

interval begins to grow longer, it may be inferred that
the storm has passed its point of nearest approach, and
is receding.   But the laws according to which thunder-
storms travel are as yet very little understood ; and it
is unsafe to assume that because the interval between
flash and peal has begun to increase after having
diminished, the storm is therefore *certainly* passing
away.   It must be in the experience of all who have
noted the circumstances of thunderstorms, that when a
storm is in the neighbourhood of the observer, the
interval between the flash and the thunder-peal will
often increase and diminish alternately several times
in succession.   It is only when the interval has become
considerable, that the danger may be assumed to have
passed away.

(From *Chambers' Journal*.)

GROWTH AND DECAY OF MIND.

And so from hour to hour we ripe and ripe,
And then from hour to hour we rot and rot,
And thereby hangs a tale.—*As You Like It*.

FEW subjects of scientific investigation are more inter-
esting than the inquiry into the various circumstances
on which mental power depends.   By mental power I
do not mean simply mental capacity, or the potential
quality of the mind, but the actual power which is the
resultant, so to speak, of mental capacity and mental
training.   The growth and development of mental power
in the individual, and the process by which, after attain-

ing a maximum of power, the mind gradually becomes less active, until in the course of time it undergoes at least a partial decay, form the special subjects of which I propose now to treat; but in order to form clear ideas on these subjects it will be necessary to consider several associated matters. In particular, it will be desirable to trace the analogy which exists between bodily and mental power, not only as respects development and decay, but with regard to the physical processes involved in their exercise.

It is now a well-established physiological fact that mental action is a distinctly physical process, depending primarily on a chemical reaction between the blood and the brain, precisely as muscular action depends primarily on a chemical reaction between the blood and the muscular tissues. Without the free circulation of blood in the brain, there can be neither thought nor sensation, neither emotions nor ideas. It necessarily follows that thought, the only form of brain action which we have here to consider, is a process not merely depending upon, but in its turn affecting, the physical condition of the brain, precisely as muscular exertion of any given kind depends on the quality of the muscles employed and affects the condition of those muscles, not at the moment only, but thereafter, conducing to their growth and development if wisely adjusted to their power, or causing waste and decay if excessive and too long continued. It is important to notice that this is not a mere analogy. The relation between thought and the con-

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dition of the brain is a reality. So far as this state-
ment affects our ideas about actually existent mental
power, it is of little importance; for it is not more
useful to announce that a man with a good brain will
possess good mental powers than to say that a muscular
man will be capable of considerable exertion. But as
it is of extreme importance to know of the relation
which exists between muscular exercise and the growth
or development of bodily strength, so it is highly
important for us to remember that the development
of mental power depends largely on the exercise of
the mind. There is a ' training ' for the brain as
well as for the body — a real physical training —
depending, like bodily training, on rules as to nourish-
ment, method of action, quantity of exercise, and so
forth.

When we thus view the matter, we at once re-
cognise the significance of relations formerly regarded
as mere analogies between mental and bodily power.
Instead of saying that as the body fails of its fair
growth and development if overtaxed in early youth,
so the mind suffers by the attempt to force it into
precocious activity, we should now say that the mind
suffers in this case in the same actual manner—that
is, by the physical deterioration of the material in and
through which it acts. Again, the old adage, ' mens
sana in corpore sano,' only needs to be changed into
' cerebrum sanum in corpore sano,' to express an actual
physical reality. The processes by which the brain
and the body are nourished, as well as those which

produce gradual exhaustion when either is employed
for a long time or on arduous work, not only correspond
with each other, but are in fact identical in their
nature; so that Jeremy Taylor anticipated a com-
paratively recent scientific discovery when he associated
mental and bodily action in the well-known apoph-
thegm, 'Every meal is a rescue from one death and
lays up for another; and while we think a thought we
die.' This is true, as Wendell Holmes well remarks,
' of the brain as of other organs: the brain can only
live by dying. We must all be born again, atom by
atom, from hour to hour, or perish all at once beyond
repair.'

And here it is desirable to explain distinctly that
the relations between mind and matter which we are
considering are not necessarily connected with any
views respecting the questions which have been at
issue between materialism and its opponents. We are
dealing here with the instrument of thought, not with
that, whatever it may be, which sets the instrument in
motion and regulates its operation. So far indeed
as there is any connection between physical researches
into the nature of the brain or its employment in
thought, and our ideas respecting the individuality
of the thinker, the evidence seems not of a na-
ture to alarm even the most cautious. Thus when
Huxley maintains that thought is ' the expression of
molecular changes in that matter of life which is the
source of our other vital phenomena,' we are still as
far as ever from knowing where resides the moving

cause to which these changes are due. We have found that the instrument of thought is moved by certain material connecting links before unrecognised; but to conclude that therefore thought is a purely material process, is no more necessarily just than it would be to conclude that the action of a steam-engine depends solely on the eccentric which causes the alternation of the steam-supply. Again, we need find nothing very venturesome in Professor Haughton's idea, that ' our successors may even dare to speculate on the changes that converted a crust of bread, or a bottle of wine, in the brain of Swift, Molière, or Shakspeare, into the conception of the gentle Glum-dalclitch, the rascally Sganarelle, or the immortal Falstaff,' seeing that it would still remain unexplained how such varying results may arise from the same material processes, or how the selfsame fuel may produce no recognisable mental results. The brain does not show in its constitution why such differences should exist. 'The lout who lies stretched on the tavern-bench,' says Wendell Holmes, 'with just mental activity enough to keep his pipe from going out, is the unconscious tenant of a laboratory where such combinations are being constantly made as never Wöhler or Berthelot could put together ; where such fabrics are woven, such colours dyed, such problems of mechanism solved, such a commerce carried on with the elements and forces of the outer universe, that the industries of all the factories and trad-ing establishments in the world are mere indo-

lence, and awkwardness, and unproductiveness, com-
pared to the miraculous activities of which his lazy
bulk is the unheeding centre.' Yet the conscious
thought of the lout remains as unlike as possible to
the conscious thought of the philosopher; nor will
crusts of bread or bottles of wine educe aught from
the lout's brain that men will think worth remember-
ing in future ages.

Moreover, we must remember that we have to deal
with facts, let the interpretation of these facts be what
it may. The relations between mental activity and
material processes affecting the substance of the brain
are matters of observation and experiment. We may
estimate the importance of such research with direct
reference to the brain as the instrument of thought,
without inquiring by what processes that instrument is
called into action. ' The piano which the master
touches,' to quote yet again from the philosophic pages
of Holmes's *Mechanism in Thought and Morals,*
' must be as thoroughly understood as the musical box
or clock which goes of itself by a spring or weight.
A slight congestion or softening of the brain shows
the least materialistic of philosophers that he must
recognise the strict dependence of mind upon its organ
in the only condition of life with which we are experi-
mentally acquainted; and what all recognise as soon
as disease forces it upon their attention, all thinkers
should recognise without waiting for such an irresis-
tible demonstration. They should see that the study
of the organ of thought, microscopically, chemically,

experimentally, in the lower animals, in individuals
and race, in health and in disease, in every aspect of
external observation, as well as by internal conscious-
ness, is just as necessary as if the mind were known to
be nothing more than a function of the brain, in the
same way as digestion is of the stomach.'

In considering the growth of the mind, however,
in these pages, it appears to me sufficient to call
attention to the physical aspect of the subject, without
entering into an account of what is known about the
physical structure of the brain and the manner in
which that structure is modified with advancing years.
Moreover, I do not think it desirable, in the limited
space available for such an essay as the present, to
discuss the various forms of mental power; indeed,
this is by no means essential where a general view of
mental growth and decay is alone in question. Pre-
cisely as we can consider the development and decay
of the bodily power without entering into a discussion
of the various forms in which that power may be mani-
fested, so we can discuss the growth of the mind with-
out considering special forms of mental action.

Nevertheless, we cannot altogether avoid such con-
siderations, simply because we must adopt some rule for
determining how to know when mental power is grow-
ing. Here, indeed, at the outset, a serious difficulty
is encountered. Certain signs of mental decay are
sufficiently obvious, but the signs which mark the
progress of the mind to its maximum degree of power,
as well as the earlier signs of gradually diminishing

mental power, are far more difficult of recognition. This is manifest when we consider that they should be more obvious, one would suppose, to the person whose mind is in question, than to any other ; whereas it is a known fact that men do not readily perceive (certainly are not ready to admit) any falling off in mental power, even when it has become very marked to others. ' I, the Professor,' says Wendell Holmes in the *Professor at the Breakfast-table*, ' am very much like other men. I shall not find out when I have used up my affinities. What a blessed thing it is that Nature, when she invented, manufactured, and patented her authors, contrived to make critics out of the chips that were left. Painful as the task is, they never fail to warn the author, in the most impressive manner, of the probabilities of failure in what he has undertaken. Sad as the necessity is to their delicate sensibilities, they never hesitate to advertise him of the decline of his powers, and to press upon him the propriety of retiring before he sinks into imbecility.' Notwithstanding the irony, which is just enough so far as it relates to ordinary criticism, there can be no question that when an author's powers are failing, his readers, and especially those who have been his most faithful followers, so to speak, devouring each of his works as it issues from his pen, begin to recognise the decrease of his powers before he is himself conscious that he is losing strength. The case of Scott may be cited as a sufficient illustration, its importance in this respect being derived from the fact that he had long been

warmly admired and enthusiastically appreciated by those who at once recognised signs of deterioration in *Count Robert of Paris* and *Castle Dangerous.*

Yet judgment is most difficult in such matters. We can readily see why no man should be skilled to detect the signs of change in his own mind, since the self-watching of the growth and decay of mind is an experiment which can be conducted but once, and which is completed only when the mind no longer has the power of grasping all the observed facts and forming a sound opinion upon them. But it is even more natural that those who follow the career of some great mind should often be misled in their judgment as to its varying power. For, it must be remembered that the conditions under which such minds are exercised, nearly always vary greatly as time proceeds. This circumstance affects chiefly the correctness of ideas formed as to the decay of mental powers, but it has its bearing also on the supposed increase of these powers. For instance, the earlier works of a young author, diffident perhaps of his strength or not quite conscious where his chief strength resides, will often be characterised by a weakness which is in no true sense indicative of want of mental power. A work by the same author when he has made for himself a name, when he knows something of the feeling of the public as to his powers, and when also he has learned to distinguish the qualities he possesses—to see where he is strong and where weak—will have an air of strength and firmness not due, or only partially due to any real growth of his

mental powers. But as I have said, and as experience
has repeatedly shown, it is in opinions formed as to the
diminution of mental power that the world is most apt
to be deceived. How commonly the remark is heard
that So-and-so has written himself out, or Such-a-one
is not the man he was, when in reality, as those know
who are intimate with the author so summarily dis-
missed, the deterioration justly enough noted is due
to circumstances in no way connected with mental
capacity. The author who has succeeded in estab-
lishing a reputation may not have (nay, very commonly
has not) the same reason for exerting his powers to the
full, as he had when he was making his reputation.
He may have less leisure, more company, new sources
of distraction, and so on. The earlier work, his *chef-
d'œuvre*, let us say, may have been produced at one
great effort, no other subject being allowed to occupy
his attention until the masterpiece had been completed
—the later and inferior work, hastily accepted as
evidence that the author's mind no longer preserves its
wonted powers, may have been written hurriedly and
piecemeal, and subjected to no jealous revision before
passing through the press.

Here I have taken literary work as affording
typical instances. But similar misapprehensions are
common in other departments of mental work. For
example, it is related that Newton, long before he was
an old man, said of himself that he could no longer
follow the reasoning of his own *Principia*, and this
has commonly been accepted as evidence that his mind
had lost power. The conclusion is an altogether unsafe

one, as every mathematician knows. It would have
been a truly wonderful circumstance if Newton had
been able, even only ten or twelve years after his
magnum opus was completed, to follow its reasoning
with satisfaction to his own mind—that is, with the
feeling that he still had that grasp of the subject which
he had possessed when, after long concentration of his
thoughts upon it, he was engaged in the task of
exhibiting a summary of his reasoning (for the
Principia is scarcely more).

I can give more than one instance in my own expe-
rience of this seeming loss of mastery over a mathe-
matical subject while in reality the mind has certainly
not deteriorated in its power of dealing with subjects
of that particular kind. I will content myself with
one. It happened that in 1869 I had occasion to
examine a mathematical subject of no very great
difficulty, but involving many associated relations, and
requiring therefore a considerable amount of close
attention. At that time I had made myself master, I
think I may say without conceit, of that particular
subject in all its details. Early in 1873 I had occasion
to resume the study of a part of the subject, in order to
reply to some questions which had been asked me.
Greatly to my annoyance I found that I had apparently
lost my grasp of it. The relations involved seemed
more complex than they had before appeared to me:
and I should there and then have dismissed the subject
(not having leisure for mere mental experiments) with
the feeling that my strength for mathematical inquiries

had diminished. But the subject chanced to be one that I could not dismiss, for though the questions directed to me might have been left unanswered, the time had come which I had assigned to myself (under certain eventualities then realised) for a complete restatement of my views, enforced and reiterated in every possible way, until a certain course depending upon them should have been adopted or else the discussion of the matter rendered useless by lapse of time. I soon found, after resuming my study of the subject, that it was far more completely within my grasp than before—in fact, on re-acquiring my knowledge of its details, the problems involved appeared to me as mere mathematical child's play.

The great difficulty in judging of the growth and development of the mind consists in the want of any reliable measure of mental strength,—any mental dynamometer, so to speak. Our competitive examinations are attempts in this direction, but very imperfect ones, as experience has long since shown. Neither acquired knowledge, nor the power of acquiring knowledge is any true measure of mental strength. The power of solving mathematical problems is not necessarily indicative even of mathematical power, far less of general mental power. The ordinary tests of classical knowledge, again, have little real relation to mental strength. It may be urged that our most eminent men have for the most part been distinguished at school or university, by either mathematical or classical knowledge, or both. This is doubtless true; but so it would be the case that.

they would have distinguished themselves above their
fellows at public school or university if the heads of
these establishments had in their wisdom set Chinese
puzzling as the primary test of merit. The powerful
mind will show its superiority (in general) in any task
that may be assigned it ; and if the test of distinction
is to be the skilful construction of Greek and Latin
verse, or readiness in treating mathematical problems,
a youth of good powers, unless he be wanting in am-
bition, will acquire the necessary qualifications even
though he has no special taste for classical or mathe-
matical learning, and is even perfectly assured that in
after life he will never pen a sapphic or set down an
equation of motion.

In passing I may note that nearly all our attempted
measurements of mind depend too much on tests of
memory. It is not recognised sufficiently that the
part which memory plays in the workings of a powerful
mind is subordinate. A good memory is a very useful
servant; nothing more. In the really difficult mental
processes, memory—at least what is commonly under-
stood by the term—plays a very unimportant part.
Of course a weak memory is an almost fatal obstacle
to effective thought; but I am not comparing the
worth of a good memory and a bad one, but of an
average memory and one exceptionally powerful. I
conceive that quite a large proportion of the most
profound thinkers are satisfied to exert their memory
very moderately. It is, in fact, a distraction from
close thought to exert the memory overmuch; and a

man engaged in the study of an abstruse subject will. commonly prefer to turn to his book-shelves for the information he requires than to tax his memory to supply it. The case resembles somewhat that of the mathematician who from time to time, as his work proceeds, requires this or that calculation to be effected. He will not leave the more engrossing questions that he has in his thoughts, to go through processes of arithmetic, but will adopt any ready resource which leaves him free to follow without check the train of his reasoning.

It would be perhaps difficult to devise any means of readily measuring mental power in examination or otherwise. The memory test is assuredly unsafe ; but it would not be easy to suggest a really reliable one. I may remark that only those experienced in the matter understand how much depends on memory in our competitive examinations. Many questions in the examination-papers apparently require the exercise of judgment rather than memory ; but those who know the text-books on which the questions are based are aware that the judgment to be written down in answer is not to be formed but to be quoted. So with mathematical problems which appear to require original conceptions for their solution : in nine cases out of ten such problems are either to be found fully solved in mathematical works, or others so nearly resembling them are dealt with that no skill is required for their solution.

I must confess that I am somewhat surprised to

find Wendell Holmes, whose opinions on such matters
are usually altogether reliable, recommending a test of
mental power depending on a quality of memory even
inferior to that usually in question in competitive ex-
aminations. 'The duration of associated impressions
on the memory differs vastly,' he says, 'as we all know,
in different individuals. But in uttering distinctly a
series of unconnected numbers or letters before a
succession of careful listeners, I have been surprised
to find how generally they break down, in trying to
repeat them, between seven and ten figures or letters;
though here and there an individual may be depended
on for a larger number. Pepys mentions a person who
could repeat sixty unconnected words, forwards or
backwards, and perform other wonderful feats of
memory; but this was a prodigy.[1] I suspect we
have in this and similar trials a very simple mental
dynamometer which may find its place in education.'
It appears to me, on the contrary, that tests of the
kind should be as little used as may be. Memory will
always have an unfair predominance in competitive
examinations; but tests which are purely mnemonic,

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[1] 'This is nothing to the story told by Seneca of himself, and still
more of a friend of his, one Portius *Latro* (*Mendax* it might be sug-
gested), or to that other relation of Muretus, about a certain young
Corsican.' The note is Holmes's; but there are authenticated instances
fully as remarkable as those here referred to. For instance, there is a
case of an American Indian who could repeat twenty or thirty lines of
Homer which had been read once to him, though he knew nothing of
the Greek language. The power of repeating *backwards* a long pas-
sage after it has been but once read is somewhat similar to that of re-
peating unconnected numbers, letters, or words. This power has been
possessed to a remarkable degree by persons in no way distinguished by
general ability.

the judgment being in no way whatever called upon,
ought not to be introduced, and should be discarded
as soon as possible where already in use.[1]

It is worthy of notice that the growth of the mind
is often accompanied by an apparent loss of power in
particular respects; and this fact is exceedingly im-
portant especially to all who desire to estimate the
condition of their own mind. The mental phenomenon
called (not very correctly) absence of mind, is often
regarded by the person experiencing it, and still more
by those observing it in him, as a proof of failing
powers. But it often, if not generally, accompanies the
increase of mental power. Newton displayed absence
of mind much more frequently and to a much more
marked degree when his powers were at their highest
than in his youth, and not only did instances become
much less frequent when he was at an advanced age,
but the opposite quality, sensitiveness to small annoy-
ances, began then to be displayed. Even an apparent
impairment of the memory is not necessarily indicative
of failing mental powers, since it is often the result of
an increased concentration of the attention on subjects
specially calling for the exercise of the highest forms

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[1] It may perhaps occur to the reader that I who write may object to
mnemonic tests because they would act unfavourably if they were
applied to my own mental qualities. The reverse is, however, the case.
I can recall competitive examinations in which I had an undue advan-
tage over others because my memory chances to be very retentive in
one particular respect :—In its general nature my memory is about
equal, I imagine, to the average, perhaps it is better than the average
for facts, and rather below the average for what is commonly called
learning 'by heart:' but it is singularly retentive for the subject
matter of passages *read o vernight.*

of mental power—as analysis, comparison, generalisa-
tion, and judgment.   I have already noted that pro-
found thinkers often refrain on occasions from exer-
cising the memory, simply to avoid the distraction of
their thoughts from the main subject of their study.
But this statement may be extended into the general
remark that the most profound students, whether of
physical science, mathematics, history, politics, or in
fine of any difficult subject of research, are apt to give
the memory less exercise than shallower thinkers.   Of
course, the memory is exerted to a considerable degree,
even in the mere marshalling of thoughts before
theories can be formed or weighed.   But the greater
part of the mental action devoted to the formation or
discussion of theories is only indirectly dependent upon
the exercise of memory.

Subject to the considerations suggested above, we
may fairly form our opinion as to the general laws of
the development of mind, by examining the lives of
distinguished men and taking the achievement of their
best work, that by which they have made their mark
in the world's history, as indicative of the epoch when
the mind had attained its greatest development.   Dr.
Beard of New York, has recently collected some
statistical results, which throw light on the subject of
mental growth, though I must note that a variety of
collateral circumstances have to be taken into account
before any sound opinion can be formed as to the
justice of Dr. Beard's conclusions.   He states that
' from an analysis of the lives of a thousand representa-

tive men in all the great branches of human effort, he
had made the discovery that the golden decade was
between thirty and forty, the silver between forty and
fifty, the brazen between twenty and thirty, the iron
between fifty and sixty. The superiority of youth and
middle life over old age in original work appears all
the greater, when we consider the fact that nearly all
the positions of honour and profit and prestige—pro-
fessorships and public stations—are in the hands of the
old.   Reputation, like money and position, is mainly
confined to the old.   Men are not widely known until
long after they have done the work that gives them
their fame.   Portraits of great men are a delusion ;
statues are lies.   They are taken when men have
become famous, which, on the average, is at least
twenty-five years after they did the work which gave
them their fame.   Original work requires enthusiasm.
If all the original work done by men under forty-five
were annihilated, the world would be reduced to
barbarism.   Men are at their best at that time when
enthusiasm and experience are most evenly balanced ;
this period on the average is from thirty-eight to forty.
After this period the law is that experience increases
but enthusiasm declines.   In the life of almost every
old man there comes a point, sooner or later, when
experience ceases to have any educating power.'

There is much that is true, but not a little that is,
to say the least, doubtful, in the above remarks.   The
children of a man's mind, like those of his body, are
commonly born when he is in the prime of life.   But.

it must not be overlooked that it is precisely because
of the original work done in earlier life that a man as
he grows older is commonly prevented from accom-
plishing any great amount of original work. Nearly
the whole of his time is necessarily occupied in
maturing the work originated earlier. And again, the
circumstance that (usually) a man finds that the work
of his earlier years remains incomplete and unsatisfac-
tory, unless the labours of many sequent years are
devoted to it, acts as a check upon original investi-
gation. This remark has no bearing, or but slight
bearing, on certain forms of literary work; but in
nearly every other department of human effort men
advanced in years find themselves indisposed to un-
dertake original research, not from any want of
power, but because they recognise the fact that
sufficient time does not remain for them to bring such
work to a satisfactory issue. They feel that they
would have to leave to others the rearing of their
mental offspring.

It cannot be questioned, however, that with old
age there comes a real physical incapacity for original
work, while the power of maturing past work remains
comparatively but little impaired. Dr. Carpenter has
shown how this may partly be explained by the
physical changes which lead in old age to the weaken-
ing of the memory; or perhaps we should rather say
that in the following passage his remarks respecting
loss of memory serve to illustrate the loss of brain
power generally, and especially of the power of

forming new ideas, in old age. 'The impairment of
the memory in old age,' he says, 'commonly shows
itself in regard to new impressions; those of the earlier
period of life not only remaining in full distinctness,
but even it would seem increasing in vividness, from
the fact that the eye is not distracted from attending
to them by the continued influx of impressions pro-
duced by passing events. The extraordinary persist-
ence of early impressions, when the mind seems almost
to have ceased to register new ones, is in remarkable
accordance with a law of nutrition I have formerly
referred to. It is when the brain is growing that
the direction of its structure can be most strongly
and persistently' (query lastingly?) 'given to it.
Thus the habits of thought come to be formed, and
those nerve-tracks laid down which (as the physiologist
believes) constitute the mechanism of association, by
the time that the brain has reached its maturity; and
the nutrition of the organ continues to keep up the
same mechanism in accordance with the demands upon
its activity, so long as it is being called into use.
Further, during the entire period of vigorous manhood,
the brain, like the muscles, may be taking on some
additional growth, either as a whole or in special parts;
new tissue being developed and kept up by the
nutritive process, in accordance with the modes of
action to which the organ is trained. And in this
manner a store of " impressions " or " traces " is ac-
cumulated, which may be brought within the " sphere
of consciousness " whenever the right suggesting-

strings are touched.   But as the nutritive activity
diminishes, the "waste" becomes more rapid than the
renovation; and it would seem that while (to use a
commercial analogy) the "old-established houses"
keep their ground, those later firms, whose basis is
less secure, are the first to crumble away—the nutri-
tive activity which yet suffices to maintain the original
structure, not being capable of keeping the subsequent
additions to it in working order.   This earlier de-
generation of later formed structures is a general
fact perfectly familiar to the physiologist.'

One of the most remarkable features of mental
development, characteristic, according to circumstances,
of mental growth and of mental decay, is the change
of taste for mental food of various kinds.   Everyone
must be conscious of the fact that books, and the
subjects of thought, lose the interest they once had,
making way for others of a different nature.   The
favourite author whose words we read and re-read with
continually fresh enjoyment in youth, appears dull and
uninteresting as the mind grows, and becomes un-
endurable in advanced years.   And this is not merely
the effect of familiarity.   I knew one who was never
tired of reading the works of a famous modern novelist
until the age of twenty-five or thereabouts, when it
chanced that he was placed in circumstances which
caused novel-reading to be an unfrequent occupation,
and in point of fact certain works of this author were
not opened by him for ten or twelve years.   He
supposed, when at the end of that time he took up one

of these works, that he should find even more than
the pleasure he formerly had in reading it, since the
story would now have something of novelty for him,
and he had once thoroughly enjoyed reading it even
when he almost knew the work by heart. But he no
longer found the work in the least interesting; the
humour seemed forced, the pathos affected, the elo-
quence false; in short, he had lost his taste for it. In
the meantime the works of another equally famous
humourist had acquired a new value in his estimation.[1]
They had formerly seemed rather heavy reading;
now, every sentence gave enjoyment. They appeared
now as books not to be merely tasted or swallowed,
but as Bacon hath it, 'to be chewed and digested.'
The change here described indicated (in accordance at
least with the accepted estimates of the novelist and
humourist in question) an increase of mental power.
But a distaste for particular writings may imply the
decay of mental power. And also, more generally,
a tendency to disparagement is a very common in-
dication of advancing mental age. 'The old brain,'
says Wendell Holmes, 'thinks the world grows worse,

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[1] Probably the best means of testing the development of one's own
mind consists in comparing the estimate formed, at different times, of
the value of some standard work. Of course different classes of writ-
ing should be employed to test different faculties of the mind. A good
general test may be found in Shakespeare's plays, and perhaps still
better in some of Shakespeare's sonnets. As the mind grows, its power
of appreciating Shakespeare increases; and the great advantage of this
particular test is that the mind cannot overgrow it. It is like the
standard by which the sergeant measures recruits, which will measure
men of all heights, not failing even when giants are brought to be
measured by it.

as the old retina thinks the eyes of needles and the
fractions in the printed sales of stocks grow smaller.'

Another singular effect of advancing years is shown
by the tendency to repetition. It is worthy of notice
that this peculiar mental phenomenon has been clearly
associated with physical deterioration of the substance
of the brain, because it may be brought about by a
blow or by disease. Wendell Holmes, speaking of this
peculiarity, remarks, 'I have known an aged person
repeat the same question five, six, or seven times,
during the same brief visit. Everybody knows the
archbishop's flavour of apoplexy in the memory as in
the other mental powers. I was once asked to see to a
woman who had just been injured in the street. On
coming to herself, "Where am I? What has hap-
pened?" she asked. "Knocked down by a horse,
ma'am; stunned a little; that is all." A pause, "while
one, with moderate haste, might count a hundred;" and
then again, "Where am I? What has happened?"
"Knocked down by a horse, ma'am; stunned a little;
that is all."' (Mr. Holmes appears to have sympathised
with the patient's mental condition.) 'Another pause,
and the same question again; and so on during the
whole time I was by her. The same tendency to repeat
a question indefinitely has been observed in returning
members of those worshipping assemblies whose
favourite hymn is, "We won't go home till morning."
Is memory, then,' he proceeds, 'a material record? Is
the brain, like the rock of the Sinaitic Valley, written
all over with inscriptions left by the long caravans of

thought, as they have passed year after year through its
mysterious recesses? When we see a distant railway-
train sliding by us in the same line, day after day, we
infer the existence of a track which guides it. So,
when some dear old friend begins that story we remem-
ber so well; switching off at the accustomed point of
digression; coming to a dead stop at the puzzling ques-
tion of chronology; off the track on the matter of its
being first or second cousin of somebody's aunt; set on
it again by the patient, listening wife, who knows it all
as she knows her well-worn wedding-ring—how can we
doubt that there is a track laid down for the story in
some permanent disposition of the thinking-marrow?'

We seem to recognise here a process of change in
the brain corresponding to that which takes place in
the body with advancing years—the induration of its
substance, so that it loses flexibility, and thus while
readily accomplishing accustomed work, is not readily
adapted for new work. Our old proverb, 'You can't
teach an old dog new tricks,' indicates coarsely enough,
but justly, the peculiarity, as well mental as bodily,
to which I refer. There is not a loss of power, but
a loss of elasticity. We see aged men working well
in the routine work to which they have become ac-
customed, but failing where there is occasion for
change either of method or of opinion. Again, one
recognises this peculiarity in the scientific worker,
whence perhaps we may regard it as a fortunate cir-
cumstance that the tendency of the aged mind accords
with its faculties, so that old men do not readily under-

take new work.   Perhaps no more remarkable instance
could be cited of the combination I refer to—the
possession of power on the one hand, and the want
of elasticity on the other—than the remarkable papers
on the universe, written by Sir W. Herschel in the
years 1817 and 1818, that is, in his seventy-ninth and
eightieth years.   We find the veteran astronomer pro-
ceeding in the path which, more than forty years
before, he had marked out for himself; but the very
steadiness and strength of purpose with which he
pursues it indicates the degree to which his mind had
lost its wonted elasticity.   In 1784 and 1785 he was
traversing a portion of the same road.   But then he
was in the prime of his powers, and accordingly we
recognise a versatility which enabled him to test and
reject the methods of research which presented them-
selves to his mind.   It was in those years that he
invented his famous method of star-gauging, which our
text-books of astronomy preposterously adopt as if it
were an established and recognised method of scientific
research.   But Herschel himself, after trying it, and
satisfying himself that it was unsound in principle, aban-
doned it altogether.   In 1817 he adopted a method of
research equally requiring to be tested, and, in my
conviction, equally incapable of standing the test; but
he now worked upon the plan he had devised, without
subjecting it to any test.   Nay, results which only a
few years before he would certainly have rejected—for
he did then actually reject results which were open to
the same objection—passed muster in 1817 and 1818,

and are recorded in his papers of those dates without comment. We may recognise another illustration of the loss of elasticity with advancing years, in the obstinacy, one may even say the perversity, with which Sir Isaac Newton, in the latter years of his life, adhered to opinions on certain points where, as has since been shown, he was unquestionably wrong, and where, had he possessed his former mental versatility, he must have perceived as much. Compare this with his conduct in earlier years, when for nineteen years he allowed his theory of gravitation to rest in abeyance—though he had fully recognised its surpassing importance—simply because certain minute details were not satisfactorily accounted for. Many other instances might be cited, were it worth while, to show how the mind commonly changes when approaching an advanced age, in a manner corresponding to that bodily change—that stiffness and want of elasticity, without any marked loss of power, which comes on with advancing years. That old age does not necessarily involve any loss of power for routine work, has been clearly shown in the lives of many eminent men of our own era. The present Astronomer Royal for England affords a remarkable illustration of the fact, as also of the associated fact that new work is not easily achieved, or an old mistake readily admitted at an advanced age.

It is well pointed out by Dr. Beard, in the lecture to which I have already referred, that ' we must not expect to find at one age the mental qualifications due to another age—we must not look for experience and

caution in youth, or for suppleness and versatility in age. We ought also to apportion to the various ages of a man the kind of work most suitable to them. Positions which require mainly enthusiasm and original work should be filled by the young and middle-aged; positions that require mainly experience and routine work, should be filled by those in mature and advanced life, or (as in clerkships) by the young who have not yet reached the golden decade. The enormous stupidity, and backwardness, and red-tapeism of all departments of governments everywhere, are partly due to the fact that they are too much controlled by age. The conservatism and inferiority of colleges are similarly explained. Some of those who control the policy of colleges — presidents and trustees—should be young and middle-aged. Journalism, on the other hand, has suffered from relative excess of youth and enthusiasm.'

Before passing from the lecture of Dr. Beard, I shall venture to quote the remarks which he makes on the evidence sometimes afforded of approaching mental decay by a decline in moral sensitiveness. 'Moral decline in old age,' he says, ' means—" Take care; for the brain is giving way." It is very frequently accompanied or preceded by sleeplessness. Decline of the moral faculties, like the decline of other functions, may be relieved, retarded, and sometimes cured by proper medical treatment, and especially by hygiene. In youth, middle age, and even in advanced age, one may 'suffer for years from disorders of the nervous system that cause derangement of some one or many

of the moral faculties, and perfectly recover. The symptoms should be taken early, and treated like any other physical disease. Our best asylums are now acting upon this principle, and with good success. Medical treatment is almost powerless without hygiene. Study the divine art of taking it easy. Men often die as trees die, slowly, and at the top first. As the moral and reasoning faculties are the highest, most complex, and most delicate development of human nature, they are the first to show signs of cerebral disease. When they begin to decay in advanced life, we are generally safe in predicting that, if these signs are neglected, other functions will sooner or later be impaired. When conscience is gone, the constitution is threatened. Everybody has observed that greediness, ill-temper, despondency, are often the first and only symptoms that disease is coming upon us. The moral nature is a delicate barometer, that foretells long beforehand the coming storm in the system. Moral decline as a symptom of cerebral disease is, to say the least, as reliable as are many of the symptoms by which physicians are accustomed to make a diagnosis of various diseases of the bodily organs. When moral is associated with mental decline in advanced life, it is almost safe to make a diagnosis of cerebral disease. . . . . Let nothing deprive us of our sleep. Early to bed and late to rise, makes the modern toiler healthy and wise. The problem for the future is to work hard, and at the same time to take it easy. The more we have to do, the more we should sleep. Let it never be

forgotten that death in the aged is more frequently a slow process than an event ; a man may begin to die ten or fifteen years before he is buried.'

When mental decay is nearing the final stage, there is a tendency to revert to the thoughts and impressions of former years, which is probably dependent on the processes by which the substance of the brain is undergoing decay. The more recent formations are the first, as we have seen, to crumble away, and the process not only brings to the surface, if we may so speak, the earlier formations—that is, the material records of earlier mental processes—but would appear to bring those parts of the cerebrum into renewed activity. Thus, as death draws near, men ' babble of green fields,' as has been beautifully said, though not by Shakespeare, of old Jack Falstaff. Or less pleasant associations may be aroused, as we see in Mrs. Grandmother Smallweed, when ' with such infantine graces as a total want of observation, memory, understanding, and intellect, and an eternal disposition to fall asleep over the fire and into it,' she ' wiled away the rosy hours ' with continual allusions to money.

The recollections aroused at the moment of death are sometimes singularly affecting. None can read without emotion the last scenes of the life of Colonel Newcome. I say the last scenes, not the last scene only, though that is the most beautiful of all. Everyone knows those last pages by heart, yet I cannot forbear from quoting a few sentences from them. "Father !" cries Clive, "do you remember Orme's

History of India?" "Orme's History, of course I do; I could repeat whole pages of it when I was a boy," says the old man, and began forthwith. " ' The two battalions advanced against each other cannonading, until the French, coming to a hollow way, imagined the English would not venture to pass it. But Major Lawrence ordered the sepoys and artillery—the sepoys and artillery to halt, and defend the convoy against the Morattoes.' Morattoes, Orme calls them. Ho! ho! I could repeat whole pages, sir." ' Later, 'Thomas Newcome began to wander more and more. He talked louder; he gave the word of command, and spoke Hindustanee, as if to his men. Then he spoke words in French rapidly, seizing a hand which was near him, and crying, "Toujours, toujours." But it was Ethel's hand which he took. . . . . . Some time afterwards, Ethel came in with a scared face to our pale group. "He is calling for you again, dear lady," she said, going up to Madame de Florac, who was still kneeling. "And just now he said he wanted Pendennis to take care of his boy. He will not know you." She hid her tears as she spoke. She went into the room, where Clive was at the bed's foot; the old man within it talked on rapidly for awhile; then again he would sigh and be still: once more I heard him say hurriedly, "Take care of him when I'm in India," and then with a heartrending voice he called out "Léonore, Léonore." She was kneeling at his side now. The patient's voice sank into faint murmurs; only a moan now and then announced that he was not asleep. At the usual

evening hour the chapel bell began to toll, and Thomas Newcome's hands outside the bed feebly beat time. And, just as the last bell struck, a peculiar sweet smile shone over his face, and he lifted up his head a little, and quickly said, " Adsum ! " and fell back.   It was the word we used at school when names were called, and lo, he whose heart was as that of a child, had answered to his name, and stood in the presence of The Master.'

Sadder than death is it, however, when the brain perishes before the body.   ' How often, alas, we see,' says Wendell Holmes, ' the mighty satirist tamed into oblivious imbecility ; the great scholar wandering without sense of time or place, among his alcoves, taking his books one by one from the shelves and fondly patting them : a child once more among his toys, but a child whose to-morrows come hungry, and not fullhanded,—come as birds of prey in the place of the sweet singers of the morning.   We must all become as little children if we live long enough ; but how blank an existence the wrinkled infant must carry into the kingdom of heaven, if the Power that gave him memory does not repeat the miracle by restoring it.'

(From the *Cornhill Magazine*, for November 1873.)

HAVE WE TWO BRAINS?

RECENTLY Dr. Brown-Sequard has brought somewhat prominently before the American scientific world the

theory—advanced many years ago by Sir Henry Hol-
land [1] and others—that we have two brains, each per-
fectly sufficient for the full performance of the mental
functions. The general opinion respecting the two
halves of the brain was formerly that the left side is
the organ serving in the movements and feeling of the
right side of the body, while *vice versâ*, the right side
serves in volition and sensation for the left side of the
body. But Dr. Brown-Sequard endeavours to show
that this is not a necessary relation; and he maintains
not only that we have two brains, but that as we make
use of only one in thought, we leave quite useless one-
half of the most important of our organs as regards
manifestations of intelligence. He points out that if
this statement be just, it is a matter of extreme im-
portance to deal carefully with the question whether
' we ought not to give education to the two sides of the
brain, or rather to the two brains.'

I would here recall the reader's attention to a
point on which I dwell in the preceding essay,[2] the
analogy between the bodily and the mental powers.
I said that the action of the brain is a process not
merely depending upon, but in its turn affecting, the

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[1] Throughout the report of Dr. Brown-Sequard's lecture, which we
have chiefly followed, the name of Sir Henry Holland appears in the
odd-looking form 'Sir Henry Olan;' rather strangely illustrating the
American belief that the letter ' h ' is unknown to Englishmen, or only
presents itself where it ought not to be ; a notion not more absurd
perhaps than the common idea in this country that every American
speaks the dialect which we pleasingly call ' Yankee.'

[2] See pages 272, 273.

physical condition of the brain, precisely as muscular
action of any given kind not only depends on the
quality of the muscles employed, but also affects the
condition of those muscles.   The analogy on which I
then dwelt, and the deductions I then pointed to,
are illustrated, and in their turn illustrate Brown-
Sequard's theory.   The bodily powers are duplex, and
very few of the bodily organs are single, though several
which are really double may appear to be single.   Now
we train both members of those twofold bodily organs
which are under the control of volition: sometimes
both equally, as in the case of the eyes and ears; some-
times with a very slight difference, as in the case of the
two legs; sometimes with a noticeable difference, as in
the case of the two arms.   Having these pairs of mem-
bers we do not think of suffering one to do all the work,
and the other to remain idle; as one eye, or one ear, or
one arm might.   But we can conceive the case of a
race of beings possessing limbs and organs such as we
have, but, through some defect in their method of train-
ing the bodily powers, using only or chiefly one member
of each pair.   To such a race it would be a new doc-
trine, and a very important one, that both members of
every pair could be used with equal or nearly equal
efficiency.   The theory, at first startling by its novelty,
would before long be established in a practical manner;
and the race would find their powers much more than
doubled by this duplication of their limbs and organs.
Now something like this is what Dr. Brown-Sequard
promises as the result of his theory if practically

adopted. ' In the remote future, perhaps, after many generations have followed the rules which he suggests for bringing both halves of the brain or both brains into operation, a community with brains more effective than ours will arise. Mental one-sidedness will disappear, and remembering that such terms imply not mere analogies between mental and bodily power but actual physical facts, we perceive that it is a matter of extreme importance to the human race to inquire on what evidence Brown-Sequard bases his ideas.

One of the proofs on which Dr. Wigan insisted, in supporting Holland's theory, was the fact that among insane persons we often recognise two different minds, either one sane and the other insane, or both insane but in different degrees. No one who has studied the literature of insanity can fail to recall instances; but I shall venture to quote in illustration a passage from an American narrative, *The Hoosier Schoolmaster,* which is based, I am assured, on an actual case which came under the notice of the author of that pleasant story.

' Ralph stood looking into a cell, where there was a man with a gay red plume in his hat and a strip of red flannel about his waist. He strutted up and down like a drill-sergeant. "I am General Jackson," he began ; "people don't believe it, but I am. I had my head shot off at Bueny Visty, and the new one that growed on isn't nigh so good as the old one; it's tater on one side. That's why they took advantage of me to shut me up. But I know some things. My head is tater

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on one side, but it's all right on t'other. And when I
know a thing in the left side of my head I know it."'
(This illustrates a point on which Dr. Wigan specially
insisted. An insane patient knows he is insane. He
will put forward insane ideas, and immediately after
having put them forward he will say, 'I know they are
insane.' 'The lunatic is at one and the same time
perfectly rational,' says Brown-Sequard, 'and perfectly
insane.' Dr. Wigan concluded, like the poor lunatic of
the Indiana workhouse, that in such cases one-half of
the brain is normal and the other half diseased; one-
half employs the faculties in a normal way, the other
half employs them in a wrong way.) The crazy pauper
is called on to give evidence, or rather he introduces him-
self to the judges, with the remark that one side of his
head being 'sound as a nut,' he 'kin give information.'
He refuses to be sworn, because 'he knows himself.'
'You see when a feller's got one side of his head tater,
he's mighty onsartain like. You don't swear me, for I
can't tell what minute the tater side'll begin to talk.
I'm talkin' out of the lef' side now and I'm all right.
But you don't swar me. But if you'll send some of
your constables out to the barn at the poor-house and
look under the hay mow in the north-east corner, you'll
find some things maybe as has been a missin' for some
time. And that a'n't out of the tater side neither.'
The exactness of the information, with the careful
references to locality and time, as also the sugges-
tion of the proper course of action—not merely 'go and
look,' but send some of your constables, &c.—all this

illustrates well the perfect contrast often existing be-
tween the two states in which a so-called lunatic exists.

There are cases, however, which are even more in-
teresting, in which two different mental conditions are
presented, neither of which presents any indication of
mental disease, except such as might be inferred from
the completeness of the gap which separates one from
the other. Dr. Brown-Sequard gives the following
account of a case of this kind. 'I saw a boy,' he says,
'at Notting Hill, in London, who had two mental lives.
In the course of the day, generally at the same time,
but not constantly, his head was seen to fall suddenly.
He remained erect, however, if he was standing, or if
sitting he remained in that position; if talking, he
stopped talking for a while; if making a movement he
stopped moving for a while; and after one or two
minutes of that state of falling forward or drooping of
the head (and he appeared as if falling asleep suddenly,
his eyes closing), immediately after that his head rose,
he started up, opening his eyes, which were now per-
fectly bright, and looking quite awake. Then, if there
was anybody in the room whom he had not previously
seen, he would ask who the person was, and why he was
not introduced to him. He had seen me a great many
times, and knew me very well. Being with him once
when one of these attacks occurred, he lifted his head
and asked his mother, "Who is this gentleman? Why
don't you introduce him to me?" His mother intro-
duced me to him. He did not know me at all. He
shook hands with me, and then I had a conversation

with him as a physician may have with a patient.   On
the next instance when I was present during an attack
of this kind, I found that he recognised me fully, and
talked of what we had spoken of in our first interview.
I ascertained from what I witnessed in these two in-
stances, and also (and chiefly, I may add) from his
mother, a very intelligent woman, that he had two
lives in reality—two mental lives—one in his ordinary
state, and another occurring after that attack of a kind
of sleep for about a minute or two, when he knew
nothing of what existed in his other life.   In his
abnormal life, the events of his normal life were for-
gotten—his ordinary life became a blank.[1]   He knew

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[1] I have been compelled slightly to modify the report of Dr.
Brown-Sequard's statement.   Though manifestly a report taken by
short-hand writers, and intended to be *verbatim*, there are places where
it is clear that either a part of a sentence has been omitted or some
words are wrongly reported.   I speak from experience in saying that
even in America, where lectures are much more carefully reported than
in England, mistakes are not uncommon.   The enterprise of the *New
York Tribune*, in taking full reports of lectures considered noteworthy,
is a well-known and most creditable feature of American journalism.
But it is a mistake to suppose that reports, even if actually *verbatim*,
can exactly represent a lecturer's meaning.   A speaker, by varieties of
inflection, emphasis, and so on, to say nothing of expression, action,
and illustration, can indicate his exact meaning, whilst using language
which written in the ordinary manner may appear indistinct and con-
fused.   Thus a most exact and carefully-prepared lecture may appear
loose and slipshod in the report.   This applies to the case where a
lecturer speaks at so moderate a rate that the short-hand writers can
secure every word, and is true even when in writing out their report
they make no mistakes—though this seldom happens, as anyone will
readily understand who is acquainted with the stenographic art.   But
the case is much worse if a lecturer is a rapid speaker.   A reporter is
compelled to omit words and sentences occasionally, and such omissions
are absolutely fatal to the effect of a lecture, regarded either as a de-
monstration or as a work of art.   Still more unfortunate will it be for a

nothing during that second state about what had
occurred in previous periods of that same condition;
but he knew full well all that had occurred then, and
his recollection of everything was as perfect then as it
was during his ordinary life concerning the ordinary
acts of that life. He had therefore two actually dis-
tinct lives, in each of which he knew everything which
belonged to the wakeful period of that life, and in
neither of which did he know anything of what had
occurred in the other. He remained in the abnormal
—or rather the less usual state, for a time which was
extremely variable—between one and three hours, and
after that he fell asleep, and got out of that state of
mind pretty much in the same way that he had got
into it. I have seen three other cases of that kind, and
as so many have fallen under the eyes of a single
medical practitioner, such cases cannot be extremely
rare.'

The circumstances just described will probably
remind the reader of cases of somnambulism, during

lecturer if he should be carried away by his subject, and pour forth
rapidly the thoughts which have come uncalled into existence. Take
the most eloquent passage from the pages of Sir J. Herschel, Tyndall,
or Huxley, strike out as many words, not quite necessary to the sense,
as shall destroy completely the flow and rhythm of the passage, omit
every third sentence, and leave the rest to be slowly read by a perplexed
student, and the effect will correspond to the report of passages which
as delivered formed the most effective part of a lecture. The result
may be a useful mental exercise, but will surely not be suggestive of
fervid eloquence. The student of such reports will do well to read as
it were between the lines, taking what appears as rather the symbol of
what was said than its actual substance. So read such reports are of
great value.

the recurrence of which the person affected recalls the
circumstances which had taken place during the pre-
vious attack, of which in the intervening wakeful state
he had been altogether oblivious. Dr. Carpenter, in
his fine work on *Mental Physiology*, records several
instances.[1] Forbes Winslow cites cases in which in-
toxication has produced similar effects ; as, for instance,
when a drunken messenger left a parcel in a place
which he was quite unable to recall when sober; but,.
becoming drunk again, remembered where it was, and
so saved his character for honesty through the loss of
his sobriety.

It may fairly be reasoned, however, that the actual
duality of the brain is not demonstrated or even sug-
gested by cases such as these last. In fact, it is not
difficult to cite evidence which, if interpreted in the
same way, would show that we have three brains, or
four or more. Thus Dr. Rush, of Philadelphia, records
that ' an Italian gentleman, who died of yellow fever
in New York, in the beginning of his illness spoke
English, in the middle of it French, but on the day of
his death only Italian.' It is manifest that the in-

[1] One of these, however, is scarcely worthy of a place in Dr. Carpen-
ter's book. I refer to the narrative at p. 596, of a servant-maid, rather
given to sleep-walking, who missed one of her combs, and charged a
fellow-servant who slept in the same room with stealing it, but one
morning awoke with the comb in her hand. ' There is no doubt,' says.
Dr. Carpenter, ' that she had put it away on a previous night without
preserving any waking remembrance of the occurrence ; and that she
had recovered it when the remembrance of its hiding-place was brought
to her by the recurrence of the state in which it had been secreted.' This
is not altogether certain. The other servant might have been able to.
give a different account of the matter.

terpretation of this case, and therefore of others of the same kind, must be very different from that which Brown-Sequard assigns, perhaps correctly, to the case of twofold mental life above related. Knowing as we do how greatly brain action depends on the circulation of the blood in the vessels of the brain, we can be at no loss to understand the cases of the former kind, without requiring a distinct brain for the different memories excited.[1] In the same way possibly we might explain the well-known case of an insane person who became sane during an attack of typhus fever at the stage when sane persons commonly become delirious, his insanity returning as the fever declined. But we seem led rather to Dr. Brown-Sequard's interpretation, by a case which recently came under discussion in our law courts, where a gentleman whose mind had become diseased was restored to sanity by a fall which was so serious in its bodily consequences as to be the subject of an action for damages.

But perhaps the most remarkable illustration of a double life is one which has been brought before the notice of the scientific world recently; some time, I believe, after Brown-Sequard's views were published. I refer to the case recently published by Dr. Mesnet, and mentioned in Dr. Huxley's remarkable

[1] 'No simple term,' says Sir Henry Holland, 'can express the various effects of accident, disease, or decay, upon this faculty, so strangely partial in their aspect, and so abrupt in the changes they undergo, that the attempt to classify them is almost as vain as the research into their cause.' The term 'dislocation of memory' was proposed by him for the phenomena of complete but temporary forgetfulness.

lecture at Belfast on the hypothesis that animals are
or may be automata. I do not purpose to quote
Huxley's account in full, as no doubt many of my
readers have already seen it, but the following facts are
necessary to show the bearing of the case on Sequard's
theory: 'A sergeant of the French army, F——,
twenty-seven years of age, was wounded at the Battle
of Bazeilles, by a ball which fractured his left
parietal bone. He ran his bayonet through the
Prussian soldier who wounded him, but almost im-
mediately his right arm became paralyzed; after
walking about two hundred yards his right leg be-
came similarly affected, and he lost his senses. When
he recovered them, three weeks afterwards, in hospital
at Mayence, the right half of the body was com-
pletely paralyzed, and remained in this condition for
a year. At present, the only trace of the paralysis
which remains is a slight weakness of the right half
of the body. Three or four months after the wound
was inflicted, periodical disturbances of the functions
of the brain made their appearance, and have con-
tinued ever since. The disturbances last from fifteen
to thirty hours, the intervals at which they occur being
from fifteen to thirty days. For four years, therefore,
the life of this man has been divided into alternating
phases, short abnormal states intervening between long
normal states.'

It is important to notice here that although this
case somewhat resembles that of Brown-Sequard's two-
lived boy, we have in the soldier's case a duality

brought about by a different cause, an accident affecting the *left* side of the head—that side, as we shall presently see, which is regarded as ordinarily if not always the seat of chief intellectual activity. The soldier's right side was paralysed, confirming the theory that so far as the bodily movements are concerned the left brain chiefly rules the right-hand organs of the body, and *vice versâ*. But the man had recovered from his paralysis, so that either the left side of the brain had been partially restored or else the right brain had acquired the power of directing the movements of the right-hand organs. But the periodical disturbances came on three or four months after the wound was inflicted, that is, more than half-a-year before the paralysis disappeared. We have, then: 1st, three weeks of unconsciousness, during which we may suppose that the left side of the brain was completely stunned (if we may apply to the brain an expression properly relating to the condition of the man); secondly, we have three months during which the man was conscious, and in his normal mental condition, but paralysed; thirdly, we have more than half a year during which a double mental life went on, but the left side of the brain was still so far affected that the right side of the body was paralysed; and lastly, we have more than three years of this double mental life, the bodily functions in the man's normal life being, it would appear, completely restored.

Assuming, then, Sequard's theory for the moment,

we have to inquire whether the man's normal con-
dition implies the action of the uninjured right
brain, or of the restored left brain, and also to deter-
mine whether the recovery from paralysis has resulted
from a more complete restoration of the left brain,
or from the right brain having acquired a power
formerly limited to the left brain. The fact that the
man's normal mental condition returned as soon as
consciousness was restored does not show that this
condition depends on the action of the left brain, for
in the unconscious state both brains were at rest.
Rather it might seem to imply that the right brain
was the brain active in the normal mental state, for
the continued paralysis of the right side showed that
the left brain was not completely restored. Yet it has
been so clearly shown by other and independent re-
searches that the left brain is the chief seat of in-
tellectual activity that we seem forced to adopt the
opinion that this man's normal condition depends on the
action of the left brain. And we may perhaps assume,
from the length of time during which the right side
remained paralyzed after the left brain had resumed
a portion of its functions, that the other portion—the
control of the right-hand organs — has never been
recovered at all by the left brain, but that the right
brain has acquired the power, a result which, as we
shall presently see, accords well with experience in other
cases.

It would almost seem, on Brown-Sequard's hypo-
thesis—though I must admit that the hypothesis

does not explain all the difficulties in this very singular case—that the right brain having assumed one set of functions belonging to the left, from time to time tries, as it were, to assume also another set of functions belonging to the left, viz. the control of mental operations, the weakened left brain passing temporarily into unconsciousness. The matter is, however, complicated by peculiarities in the bodily state, and in sensorial relations during the abnormal condition. The whole case is, in fact, replete with difficulties, as Professor Huxley well points out,[1] and it seems to me these difficulties are not diminished by Brown-Sequard's theory. .

Let us consider some of the facts of the man's twofold life :—' In the periods of normal life the ex-sergeant's health is perfect ; he is intelligent and kindly, and performs satisfactorily the duties of a hospital attendant. The commencement of the abnormal state is ushered in by uneasiness and a sense of weight about the forehead, which the patient com-

[1] I may in passing note that the case of Brown-Sequard's double-lived boy throws some light on the question whether the soldier is conscious in his abnormal state. Professor Huxley says justly that it is impossible to prove whether F. is conscious or not, because in his abnormal condition he does not possess the power of describing his condition. But the two conditions of the boy's life were not distinguished in this way, for he was perfectly rational, and could describe his sensations in both conditions. The only evidence we can have of any other person's consciousness was afforded by this boy during his abnormal state. But what strange thoughts are suggested by this twofold consciousness—or, rather (for twofold consciousness is intelligible enough), by this alternate unconsciousness. To the boy in one state, what was the other life? *Whose* was the life of which he was unconscious?

pares to the constriction of a circle of iron ; and after
its termination he complains for some hours of dulness
and heaviness of the head. But the transition from
the normal to the abnormal state takes place in a few
minutes, without convulsions or cries, and without
anything to indicate the change to a bystander. His
movements remain free and his expression calm, except
for a contraction of the brow, an incessant movement
of the eyeballs, and a chewing motion of the jaws. The
eyes are wide open, and their pupils dilated. If the
man happens to be in a place to which he is accustomed
he walks about as usual ; but if he is in a new place,
or if obstacles are intentionally placed in his way,
he stumbles gently against them, stops, and then,
feeling over the objects with his hands, passes on one
side of them. He offers no resistance to any change
of direction which may be impressed upon him, or to
the forcible acceleration or retardation of his movements.
He eats, drinks, smokes, walks about, dresses and un-
dresses himself, rises and goes to bed at the accustomed
hours. Nevertheless, pins may be run into his body,
or strong electric shocks sent through it without caus-
ing the least indication of pain ; no odorous substance,
pleasant or unpleasant, makes the least impression ; he
eats and drinks with avidity whatever is offered, and
takes asafœtida, or vinegar, or quinine, as readily as
water ; no noise affects him ; and light influences
him only under certain conditions. Dr. Mesnet re-
marks that the sense of touch alone seems to persist,
and indeed to be more acute and delicate than in

the normal state; and it is by means of the nerves
of touch, almost exclusively, that his organism is
brought into relation with the outer world.'

Such are the general phenomena presented by this
curious case. As respects the details of the man's beha-
viour under particular circumstances, I refer the reader
to Professor Huxley's paper in the *Fortnightly Review*
for November 1874. But one peculiarity is so note-
worthy, and rightly understood gives so special an
interest to Brown-Sequard's hypothesis, that I must
quote it at length, together with the significant re-
marks with which Professor Huxley introduces the
subject. 'Those,' he says, 'who have had occasion
to become acquainted with the phenomena of som-
nambulism and mesmerism will be struck with the
close parallel which they present to the proceedings
of F. in his abnormal state. But the great value of
Dr. Mesnet's observations lies in the fact that the
abnormal condition is traceable to a definite injury
of the brain, and that the circumstances are such as
to keep us clear of the cloud of voluntary and in-
voluntary fictions in which the truth is too often
smothered in such cases. In the unfortunate subjects
of such abnormal conditions of the brain, the dis-
turbance of the sensory and intellectual faculties is
not unfrequently accompanied by a perturbation of
the moral nature which may manifest itself in a
most astonishing love of lying for its own sake. And
in this respect, also, F.'s case is singularly instructive,
for although in his normal state he is a perfectly

honest man, in his abnormal condition he is an in-
veterate thief, stealing and hiding away whatever he
can lay hands on, with much dexterity, and with an
absurd indifference as to whether the property is his
own or not.   Hoffmann's terrible conception of the
' Doppelt-gänger' is realised by men in this state,
who live two lives, in the one of which they may
be guilty of the most criminal acts, while in the
other they are eminently virtuous and respectable.
Neither life knows anything of the other.   Dr. Mesnet
states that he has watched a man in his abnormal
state elaborately prepare to hang himself, and has
let him go on' (!) ' until asphyxia set in, when he
cut him down.   But on passing into the normal
state the would-be suicide was wholly ignorant of
what had happened.'

If Wigan and Sequard are right in regarding the
changes of opinion with which most of us are familiar
as differing only in degree from the duality of a
lunatic's mind who has sane and insane periods, and
mental indecision as differing only in degree from the
case of a lunatic who 'is of two minds,' knowing
that what he says is insane, a curious subject of
speculation arises in the consideration of the pos-
sible duality of the moral nature.   The promptings
of evil and the voice of conscience resisting these
promptings, present themselves as the operation of the
two brains, one less instructed and worse trained than
the other.   'Conversion' is presented to us as a
physical process, bringing the better trained brain into

action in such sort as to be the only or chief guide of the man's actions.

Passing, however, from thoughts such as these to the reasoning which must determine our acceptance of the theory suggesting them, let us consider what evidence we have to show that a real difference exists between the right and left brains.

It has been shown that the faculty of speech depends either wholly or mainly on the left side of the brain. A lesion in a particular region of this side produces the loss of the faculty of expressing ideas by spoken words. Out of more than a hundred cases of this peculiar disease—*aphasia*—only one is known (and that case is doubtful) in which the right side of the brain was diseased. This seems to show that the two sides of the brain are distinct one from the other. At first sight, however, the idea might suggest itself that this evidence tended to prove that the two portions of the brain discharge supplementary functions. If the left side thus perform duties with which the right side has nothing to do, presumably the right side may perform duties from which the left side is free. This, indeed, would appear to be the case; but Brown-Sequard's position is that this is not a necessary distinction; but the result of habit, unconsciously exercised of course, since (as yet, at any rate) we do not possess the power of deciding that we will use this or that side of the brain. He maintains that the left brain is used in speech, as the right hand is used in writing; that a disease in the particular part of the

left brain on which speech depends, causes *aphasia,*
precisely as a disease of the right hand destroys the
power of writing (until the left hand has been trained
to the work); and that by training both brains we
should render this particular form of cerebral disease
less likely to cause loss of speech, much in the same.
way that by training both hands to write, we should
diminish the chance of any such cause as disease or
accident depriving us of the power of writing.

Brown-Sequard further maintains that where the
power of articulation is lost, it is not the mere
power of moving the muscles of the tongue, larynx or
chest, which is lost, but the memory of the mode of
directing the movements of those muscles. In many
cases, he says, 'a patient could move the tongue in
any direction, could move the larynx, and utter
sounds very well; but could not articulate, the mental
part of the mechanical act being lost, not the
mechanical action itself.'

Sight affords evidence that the distinct action of
the two sides of the brain is not incompatible with
the completeness of the power possessed by either.
Wollaston held that the right side of the base of
the brain is the centre for sight in the two right halves
of the eye,—that is, the half of the right eye towards
the temple, and the half of the left eye towards the
nose; while the left side of the base of the brain is
the centre for sight in the two other halves—the
outer half of the left eye and the inner half of the

right eye. If this were so, the two halves of the brain would be, so far as sight is concerned, absolutely supplementary to each other, insomuch that a disease of either half of the brain would render sight imperfect. It is not altogether true, however, as Brown-Sequard states, that only one half of each object would be seen, for the whole of an object may fall on either half of the retina. But objects looked at full front would thus be divided. If the left side of the brain were affected, the left halves of the eyes would act imperfectly, that is, the left halves of the visual field within the eye; so that, in point of fact, objects towards the observer's right would be unseen; and *vice versâ*. Wollaston himself was troubled occasionally by a defect of this kind. Trying one day to read the name of an instrument—the barometer—he could read only ' meter,' the other part of the word, ' baro,' being invisible. Agassiz was similarly affected. And many patients who are afflicted with certain disorders of movement implying brain disease have the same trouble—they see only half of objects towards which the eyes are directly turned. Nor is this the only evidence which at a first view seems to demonstrate Wollaston's theory. If the theory were true we should expect to find that when only a small part of one side of the brain—or rather, of that region on which sight depends—was affected, then only the half of one eye would be deprived of sight. This has been found to be the case. And naturally, we should expect that if the other part of the region (of the same side of the brain) were affected,

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then the corresponding half of the other eye, and that half only, would be deprived of sight. This also has been found to be the case. Nevertheless, Wollaston's theory has to be abandoned because it does not account for all the facts, and is opposed by three decisive facts at least.[1] It has been shown in many instances that a disease in one half of the brain will produce complete loss of sight, (i.) of the two halves of the eye on the same side as the diseased brain; or (ii.) of the two halves of the eye on the opposite side; or (iii.) of the two halves of both eyes. Manifestly then there is no necessary association between either side of the brain and the sight of either eye, or of the two halves of either eye. Each side of the brain possesses apparently the *potentiality* of rendering sight perfect for both eyes. Admitting this, it is clearly a point of great importance to inquire whether both sides of the brain, or the two brains, may not each be trained to discharge this duty; for the disease of either would no longer destroy or seriously impair the power of sight.

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[1] It is singular how seldom the true rules which should guide us in selecting and rejecting theories are recognised and understood. Over and over again we see it assumed, if not stated, that that theory which accounts for the greatest number of facts is to be adopted as the most probable. This is not by any means the case. The true theory must, in reality, accord with *all* the facts, though we may not be able to show that it does. Now if a theory accounts for several of the facts, and is not opposed by a single one, it has a much better claim to be adopted provisionally as the most probable, than another theory which accounts for a greater number of facts, or even for all the known facts save one, but is manifestly opposed by one fact. This is a rule of the utmost importance in science, because often it enables us to select the true theory, not by overpowering testimony of evidence in its favour, but by consecutively rejecting all other possible theories.

The next point considered by Brown-Sequard is that of gesture. The left side of the brain chiefly controls the gestures, and this for the simple reason that the left side of the brain guides chiefly the movements of the right side of the body, and it is chiefly with the right arm that gestures are made. But it also appears likely, from certain pathological facts, that even the motion of the left arm, so far as gestures are concerned, depends on the action of the left side of the brain; for it is found that patients who have the left side of the brain diseased commonly lose the faculty of making appropriate gestures with either the right or the left arm. It has, however, happened in a few cases that the disease of the right side of the brain has led to a loss of the power of making gestures. It need hardly be remarked that this exception no more opposes itself to the general theory of the duality of the brain than does the fact that a certain proportion of persons are left-handed, or one may say left-sided.

There is a difficulty in determining how far writing depends on the left side of the brain, because disease of that side is not uncommonly accompanied by paralysis of the right arm and hand, and in such cases we cannot determine whether the power of writing is lost on account of a real loss of memory of the relation between written symbols and the ideas they express, or simply through the effects of paralysis. However, it very seldom happens that paralysed patients have lost altogether the use of the fingers and are unable to make the least sign. In fact it is found that

in many cases they can imitate writing placed before them (oftener if the handwriting resembles their own) while they are unable from memory to write anything, or at all events to express ideas by writing. The disease is called *agraphia.* In many patients suffering from this disease the right arm is perfectly free from any sign of paralysis, but a portion of the left side of the brain has been diseased. It would appear therefore that written language, like spoken language, depends on the left side of the brain.

It is also known that the power of reasoning depends on the left side of the brain more than on the right. In cases of insanity the left side of the brain has more frequently been found to be diseased than the right side.

We see, then, that to the left brain we must assign the chief control over speech, writing, and gesture— the methods, that is, of expressing ideas. This side also seems principally concerned in the process of reasoning ; and besides these special functions, we must assign to the left side of the brain the principal control over the motions and organs of the right side of the body.

The right side of the brain in turn possesses its special functions. It serves chiefly to the emotional manifestations, including those called hysterical, and also to the needs of the body as respects nutrition.[1]   It also, of

[1] The evidence adduced by Dr. Brown-Sequard respecting the special functions of the right side of the brain is chiefly derived from his medical experience, and would, therefore, not be altogether suitable to

course, possesses a function corresponding to the control of the left side of the brain over the bodily organs, the right side having principal control over the movements and organs of the left side of the body.

And now for the practical application of these facts.

If the difference which exists between the two sides of the brain depended on a radical difference in their structure, it would, of course, be impossible to bring about any change. The facts I have cited would be interesting, but they would have no practical application, however thoroughly they might be demonstrated. We recognise clearly the difference between the functions of the eye and those of the ear, between the office of the legs and that of the arms ; but we do not inquire whether both the eye and the ear might be trained to perform the same duties, nor do we practise walking on our hands, or grasping objects with our feet. But it is manifest that a useful purpose might be

these pages—or rather, its force would not be so clearly recognised as that of the evidence relating to language and gesture. It appears that ulceration of the lungs or liver, hæmorrhage and sudden inflammation, may result more or less directly from irritation, and that in such cases it has chiefly been the right side of the brain which has been affected. Among 121 cases of paralysis, caused by hysteria, 97 were found associated with disease of the right side of the brain, and only 24 with disease of the left side. It is also well known that paralysis is more common on the left side of the body than on the right side, which corresponds to the fact that the right side of the brain is more commonly diseased in the manner which results in paralysis. He cites other medical evidence in support of the theory that the right side of the brain is chiefly concerned in the nutrition of the various organs of the body.

served by calling to any person's attention the fact,
if such it should be, that he uses one or other eye more
frequently than the other, or for different purposes,
and that his general powers of sight would be improved
if he accustomed both eyes to the same amount and
kind of work.[1]   Similarly of the ears.   Again some
persons are *too* right-handed (I question, indeed,
whether one-handedness, whether right or left be
chiefly employed, does not in all cases involve a loss of
power).   In all such cases it is probable that careful
training, especially if begun in early life, by tending
to equalise the work of each member of each pair
of organs, might add considerably to the general

[1] Perhaps in some instances the reverse may be the case—though we
question whether many would care to have one eye specially suited for
one kind of work, and the other eye for a different kind.   This is not an
imaginary case.   It is much more common than many suppose, for
the two eyes to differ in focal length; and if the difference is not
early noticed, it is apt to increase, each eye being used for the work
to which it is best suited.   I suppose that a marked difference be-
tween my own eyes attained its present extent in this way, though
the difference was probably considerable in childhood.   It is now so
great that my left eye is scarcely used at all (and is almost useless
for ordinary vision), being very near-sighted, but is almost micro-
scopic for near objects; while the right eye is not used at all on
examining minute objects, and very little in reading, but is of average
power for distant objects.   To use both has become impossible, and
may have always been so.   The difference, however, was not noticed
until I was about 18 years of age.   That it existed in boyhood
to a marked degree, I consider to be proved by the difficulty I
experienced in acquiring skill in such games as cricket, rackets, fives,
billiards, &c., where ready and exact judgment of distances is required.
I believe that in almost every instance when a boy shows a marked
want of skill in such games—while apt in others—it will be found that
one eye differs so much in focal length from the other as to be little
used.

powers of the body. It is something of this sort that Brown-Sequard hopes to attain for the brain; in fact, it is by this very process that he hopes to bring into action the full powers of this dual organ.

He remarks that ' every organ which is put in use for a certain function gets developed, and more apt or ready to perform that function. Indeed, the brain shows this in point of mere size. For the left side of the brain, which is used most, is larger than the right side. The left side of the brain also receives a great deal more blood than the right side, because its action preponderates, and every organ that acts much receives more blood. As regards the influence of action on the brain, there is a fact which hatters know very well. If a person is accustomed for many years of adult life— say from 20 up to 40 or more—to go to the same hatter, the hatter will find after a time that he has to enlarge the hat of his customer; and, indeed, a person advanced in years, even having passed 56, as your lecturer has, may have a chance to observe such a change. There is no period of six months that has passed that I have not found my hat, if neglected and put aside, has become too small. The head growing is very strong proof that the brain grows also. Action is a means of increasing size. It is also a means of developing power. I have no doubt that a good many among you have observed that, after paying great attention to a subject, they have not only acquired knowledge on that subject, but become much better able to solve questions relating to that subject—that

having developed the part of the brain which has been
used for the acts performed, that part has become far
better able to perform the duties demanded of it.'

The superior size, therefore, of the left side of the
brain, as well as the fact that it receives a larger share
of blood than the right, show that it is predominant in
our system. This fact is also shown by the prevalence
of right-handedness among all races of men. There is
no left-handed race among all the races that people the
world.[1] But also, the left-handed individuals of every
race have the brain correspondingly unequal, only that
in their case the right side of the brain is more de-
veloped, and that side, instead of the left, controls the
faculty of expressing ideas, whether by language or by
gesture, and acts chiefly in intellectual operations. The
connection between greater development of the brain
and the control of reason, and its expression, by the
side of the brain so developed, seems conclusively
established. The side of the brain which chiefly guides
our actions has the greater mass of grey matter, the

[1] Right-sidedness extends even to lower races, though there are few
cases in which we have the means of determining it. Birds, and espe-
cially parrots, show right-sidedness. Dr. W. Ogle has found that few
parrots perch on the left leg. Now parrots have that part at least of
the faculty of speech, which depends on the memory of successive
sounds, and of the method of reproducing such imitation of them as a
parrot's powers permit ; and it is remarkable that their left brain re-
ceives more blood and is better developed than the right brain. So far
Dr. Brown-Sequard on this point. It may be questioned whether
monkeys show any tendency to right-handedness ; my own recollections
of monkey gestures certainly suggest no preference of the kind. Here is
a field for observation and inquiry among our zoological professors
when young Guy Fawkes has passed through his teething.

greater number of convolutions, the most plentiful
supply of blood.

Now it appears certain that the greater develop-
ment of the left side of the brain, and consequently, if
the inferences just drawn are sound, the chief use of
that side in reason, language, and gesture, is brought
about by actions under the control of will.   We exer-
cise most the right side of the body, hence the left side
of the brain becomes better developed than the right,
and hence, therefore, it assumes the function of con-
trolling intellectual processes and their expression.   If,.
of set purpose, we exercised equally both sides of the
body, if in particular we employed the organs on the
left side in processes at present chiefly or wholly man-
aged by those on the right, would not the two sides of
the brain become equally developed, and might not
both become capable of controlling the reasoning
faculties?   On this point we have evidence which is
well worth considering, even if it cannot be regarded
as decisive.

Cases have occurred in which the left side of a
child's brain has become diseased before the child has
learned to talk.   In such cases the child has learned to
talk as well, or nearly as well, as if the left side of the
brain had been sound.   Now, if in such cases the child
had been born of left-handed parents, we could regard
the result as depending on the hereditary transmission
of exceptional powers to the right side of the brain..
But no such explanation has been available.   In most
instances, certainly (in all according to Brown-Sequard's

belief) the parents of these children were right-handed. In fact, the circumstance that these children, besides being able to speak, could make use of all the members of the right side of the body (though the left side of the brain, which usually controls the movements of those members, was diseased), shows that the right side of the brain had assumed powers not ordinarily belonging to it. The children, however, as might be expected, were left-handed, the left side of the body being governed as the special province of the right brain, and the right side only because the disease of the left brain forced on the right brain the duty of governing the right side of the body, as well as that of controlling reason, speech, and gesture.

The next point cited by Dr. Brown-Sequard does not seem quite so clearly favourable to his views; in fact it appears to me to *suggest* a rather strong argument against the hope which he entertains that the general mental powers may be improved by exercising both sides of the brain in the same kind of work. He points out that very few left-handed persons have learned to write with the left hand, and that those who can write with that hand do not write nearly so well with it as with the right hand. 'Therefore,' he says, ' the left side of the brain, even in persons who are left-handed naturally (so that the right side of the brain controls the reasoning faculties and their expression) can be so educated that the right hand, which that side of the brain controls, produces a better hand-writing than that by the left hand, though this is controlled by

the better developed brain.' This certainly seems to
show the possibility of training one side of the brain to
do a part of the work appertaining in the ordinary
course of things to the other; but the inferiority of the
writing with the left hand is rather an awkward result
so far as Brown-Sequard's hopes are concerned. For it
looks very much as though the habit of writing with
the right hand, which in the case of a left-handed per-
son is in fact the wrong hand for writing with, rendered
the right brain less fit to control that special depart-
ment of its duties (for a left-handed person) which
relates to the expression of ideas by writing. Now it
may be a very useful thing to acquire true duality of
brain-power, if the ordinarily less-used side of the brain,
for any particular action, does not acquire full power
for that function at the expense of the other side; but
otherwise the advantage is not so obvious. If we could
train the left arm to be as skilful as the right, without
losing the skill of the right arm, we should willingly
take the proper measures; but merely to shift the skill
from one arm to the other would lead to no advantage,
even if we could be quite sure that it would involve no
loss. And, as I have said, this particular argument
suggests a test which can hardly be expected to favour
Brown-Sequard's theory. Left-handed persons are con-
tinually exercising their left or less developed brain in
work properly appertaining to the right brain (in this
case). Accordingly, with them the two brains are
more equally exercised than in the case of right-handed
persons. But are the left-handed observed to be ordi-

narily of better balanced mind than the right-handed ?
Are they less liable to paralysis of one side of the body,
through having each brain readier to discharge the
functions of the other ?   It seems to me that if neither
of these relations exists, and I can scarcely suppose
that either could exist without having long since been
recognised, we may regard Brown-Sequard's theories as
interesting perhaps, and even trustworthy, but we can
scarcely place much reliance on the hopes which he
bases upon those theories.

His next argument seems somewhat more to the
purpose.   Right-sidedness affects the arms, as we know,
much more than the legs.   It is presumable, therefore,
that there is not so special a relation between the more
developed left brain and the action of the right leg,
which is only the equal of the left leg, as there is
between the left brain and .the more skilful of the two
arms.   In other words, we may assume that both brains
control both legs.   In fact, if, by equalising the prac-
tice of the two arms we are to bring the two brains not
only into more equal operation, but into combined
action on each arm, it would appear that the equal
exercise of the two legs *ought* to have resulted in com-
bining the action of the two brains so far as the control
of the lower limbs is concerned.   So that we not only
may ' infer this state of the two brains from the observed
powers of the two legs,' but unless we do assume this,
the hopes entertained by Brown-Sequard must be
regarded as to some degree negatived.   Now if the
brains do thus act in combination in controlling the

lower limbs, it is clear that the complete paralysis of a
leg ought not to be so common as the complete paralysis
of an arm, for an arm would be paralysed if only one
side of the brain were affected, but for a leg to be
paralysed both sides of the brain must be affected. Dr.
Brown-Sequard states that this is the case at least to
this degree, that 'it is exceedingly rare that the leg is
affected in the same degree by paralysis as the arm.' [1]

The hope entertained by Dr. Brown-Sequard is that
by teaching our children to use both sides of the body
equally, the two sides of the brain may be brought into
more uniform action. 'If you have been convinced by
the arguments I have given that we have two brains,'
he says, 'it is clear that we ought to develop both of
them, and I can say at any rate as much as this, there
is a chance,—I could not say more, but at least there
is a chance,—that if we develop the movements of the
two sides of the body, the two arms and the two legs,
one just as much as the other, the two sides of the brain
will then be developed one as much as the other as
respects the mental faculties also.' There is a connec-
tion between the development of the brain as regards
the mental faculties and the development as regards
leading movements on one side of the body : therefore,
Brown-Sequard considers that if we train the left side
of the body as carefully as we are in the habit of train-

[1] I do not feel quite sure that I have rightly dealt with the
Doctor's argument in this case; because he has presented it very
briefly, with the remark that it cannot be understood well except by
medical men, and my explanation, not requiring a medical training on
the reader's part, is therefore presumably inexact.

ing the right, there is a chance that we should have
two brains as respects mental functions instead of one
as at present. Since in cases of disease of the left side
of the brain the right side can be trained to exercise
all the functions usually performed by the left side, it
seems reasonable to hope that we can do as much for
the right side of the brain when the left side is sound.
Dr. Brown-Sequard suggests, therefore, that no child
shall be allowed to remain either right-sided or left-sided,
but be initiated as early as possible into two-sided ways.
' One day or one week it would be one arm which would
be employed for certain things, such as writing, cutting
meat, or putting a spoon or fork in the mouth, and so
on. In this way it would be very easy to obtain a
great deal, if not all. We know that even adults can
come to make use of their left arm. A person who has
lost his right arm can learn to write (with difficulty, it
is true, because in adult life it is much more difficult
to produce these effects than in children), and the left
arm can be used in a great variety of ways by persons
who wish to make use of it.' . . . . ' There is also
another fact as regards the power of training. Even in
adults, who have lost the power of speech from disease
of the left side of the brain, it is possible to train the
patient to speak, and most likely then by the use of the
right side of the brain, the left side of those patients,
with great difficulty, will come to learn. The same
teaching we employ with a child learning to speak
should be employed to teach an adult who has lost the
power of speech. So also as regards gesture and other

ways of expressing ideas. I have trained some patients to make gestures with the left arm who had lost the power of gesture with the right, and who were quite uncomfortable because their left arm, when they tried to move it, at times moved in quite an irregular way, and by no means in harmony with their intention. There is a power of training, therefore, for adults; and therefore that power no doubt exists to a still greater degree in the case of children; and as we know that we can make a child, who is naturally left-handed, come to be right-handed, so we can make a child, who is naturally right-handed, come to be left-handed as well. The great point should be to develop equally the two sides of the body, in the hope that by so doing the two sides of the brain, or the two brains, may be brought into harmonious action, not only as respects bodily, but also as respects mental functions.'

I have thus brought before the reader the hopes, as well as the theoretical views, of Dr. Brown-Sequard. I must say in conclusion that although for my own part I do not regard his hopes as altogether well based, believing, in fact, that many familiar experiences are against them, I attach great importance to the theoretical considerations to which he directs attention. We may not be able to increase general mental power, and still less to double mental power by calling the two sides of the brain into combined activity (as respects intellectual processes), yet if we recognise the duality of the brain in this respect we may find it possible to assist the reasoning side of the brain in

other ways. For instance, it may be found that by
considering the facts to which Brown-Sequard has
called attention we can more clearly understand the
advantage which the student has long been known to
derive from special forms of relaxation. It may, for
instance, be a specially desirable change for the student
to have his emotions called into play, because the over-
worked reasoning part of the brain obtains in that way
a more complete rest. When either side of the head
is suffering from temporary ailments, as in migraine
(hemikranion), special forms of mental[1] or bodily
exercise may be found useful to remove or alleviate the
suffering. And it cannot be but that, in studying the
effects of such experiments as Brown-Sequard suggests,
light would be thrown on the interesting and perplexing

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[1] An experience of my own seems to suggest this as possible.
On one occasion, when I was about to deliver a lecture to a large
audience (the largest I had ever addressed, in fact, and computed at
nearly 3,000), I was suffering from a headache affecting the right side
of the head so severely that the slightest movement caused intense pain,
and every breathing was responded to by a dismal throbbing of the
brain. The headache was not occasioned by excitement, but was con-
nected with a general disturbance of the system from a severe cold, and
was intensified by a journey from Chicago to New York (where the
lecture was delivered), completed only two or three hours before the
lecture began. During the first ten minutes of the address the pain was
very great indeed, and was rendered more severe by the effort required
in addressing so large a meeting with a voice affected by catarrh. But
from that time the pain grew less, and at the end of the lecture no
trace of it remained. The headache did not return after the lecture was
over; in fact, the rest of the evening was passed in such manifest enjoy-
ment of pleasant converse at the Century Club, that several 'Centu-
rions' who had heard the lecture must in all probability have found it
difficult to recoconcile the circumstance with the lecturer's statement
about his illness. [Ah! goodly fellowship of 'Centurions'! where else
in the world are so many genial souls gathered together? and where
else in the world does the stranger receive so warm a greeting?]

subject of the brain's action in relation to consciousness and volition.   If in addition to such useful results as these it should be found that by careful training on Brown-Sequard's plan the duality of the brain can be made a source of increased mental power, or of better mental balance, or of readier decision, so much the better.   The progress of science calls for increased mental activity.   We want more powerful brains than served our forefathers, for we try to grapple with more difficult questions.   The idea is at least pleasing to contemplate, though I fear it is based as yet on no very firm foundation, that as binocular vision gives a power of determining the true position of objects which the single eye does not possess, so bi-cerebral thought may supply a mental parallax enabling men to obtain juster views of the various subjects of their thoughts than they can obtain at present by mental processes which are known to be one-sided.

(From the ' Cornhill Magazine ' for September 1874.)

ON SOME STRANGE MENTAL FEATS.

WHEN we consider the connection between mental development and the progress of the human race, we cannot fail to recognise the importance of researches into mental habitudes, in individuals and in races. The questions which have been so much discussed lately as to the automatism of mental action, the laws of cere -

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bral heredity, the relation between mental and physical disease, the extent to which responsibility depends on the condition of the brain, and the like, have a much wider interest than many imagine. Our insight into the past history of mankind, our views respecting passing events, our hopes or fears as to the future, depend in no small degree on the opinion we form respecting laws of cerebral action and cerebral development. We cannot rightly understand the conduct of man towards man, of nation towards nation, of race towards race, until we begin to understand the nature of the organ which rules, directly or indirectly, every conscious action of each individual person—affecting not only the reasoning faculties but the feelings and emotions, not only the mental but the moral qualities.

My object in the present essay is to consider certain mental feats which seem calculated to throw light on the operations of that wonderful organ on which our consciousness, in the widest acceptation of the term, depends. In particular, they seem to indicate cerebral capabilities, uncommon at present, but which may one day be possessed by many. I do not, however, propose to inquire here what prospect there is that hereafter the human race may possess greater mental energy than at present, whether as respects average intellectual development or the mental powers of those who stand above their contemporaries as the great thinkers of their day,[1] but simply to

[1] We must not be misled by the consideration that we do not recognize, in the few past centuries over which our survey extends, a law of

discuss, and if possible explain, certain remarkable mental feats.

We may begin conveniently by considering some illustrations of exceptional power in the form of mental activity least likely to deceive us—aptitude in dealing with numbers. It is well remarked by Dr. Carpenter, that this quality is so completely a product of culture that we can trace pretty clearly the history of its development. ' The definite ideas which we now form of *numbers*,' he proceeds, ' and of the *relations of numbers*, are the products of intellectual operations based on experience. There are savages at the present time who cannot count beyond five; and even among races that have attained to a considerable proficiency in the arts of life, the range of numerical power seems

continuous mental development, illustrated by the increasing great-ness of the great men of successive ages; for, in the first place, if the average of intellectual development is steadily increasing, the men of exceptional mental power must appear to stand less conspicuously above that higher level than the great men of former ages above the lower average of their day. And again, the periods with which we have to deal are probably short compared with those which may be expected (when the laws of mental development come to be understood) to sepa-rate the appearance of exceptionally great minds. We carry back our thoughts to the last of the great ones in each department of mental action ; and even if we do not exaggerate his relative elevation above his contemporaries, as we are apt to do, or overlook (as we are equally apt to do) the elevation of the great minds of our own time, we still forget that, in the steady rising of the mighty tide of mental progress, the waves successively flowing in above the tide-line may be separated in time by intervals of many generations, and a greater wave may be followed by several lesser ones, before another like itself, but riding on a higher sea, flows higher still above the shore-line which separates the unknown from the known.

extremely low. . . . . . The science of Arithmetic, as at present existing, may be regarded as the accumulated *product* of. the intellectual ability of successive generations, each generation building up some addition to the knowledge which it has received from its predecessor. But it can scarcely be questioned by any observant person that an *aptitude* for the apprehension of numerical ideas has come to be embodied in the congenital constitution of races which have long cultivated this branch of knowledge ; so that it is far easier to teach arithmetic to the child of an educated stock than it would be to a young Yanco of the Amazons, who, according to La Condamine, can count no higher than *three*, his name for which is Poettarrarorincoaroac.'

As an illustration of congenital aptitude for dealing with numbers, Dr. Carpenter takes the case of Zerah Colburn ; and in this I shall follow him, though, as will presently appear, I differ from him as to the significance of that case, the true interpretation of which I believe to be far simpler, but to promise much less, than that adopted by Francis Baily and quoted with approval by Carpenter.

Let us first consider the facts of this remarkable case—

Zerah Colburn was the son of an American peasant or small farmer. When he was not yet six years of age, he surprised his father by his readiness in multiplying numbers and solving other simple arithmetical problems. He was brought to London in 1812, when only eight years old, and his powers were tested by Francis Baily

and other skilful mathematicians. From Carpenter's synopsis of the experiments thus made the following account is taken, technical expressions being as far as possible eliminated (or not used until explained):—

He would multiply any number less than 10 into itself successively nine times, giving the results (by actual multiplication, not from memory) faster than the person appointed to record them could set them down. He multiplied 8 into itself fifteen times, or, in technical terms, raised it to the sixteenth power; and the result, consisting of fifteen digits, was right in every figure. He raised some numbers of two figures as high as the eighth power, but found a difficulty in proceeding when the result contained a great number of figures.

So far there is nothing which cannot be explained (or which could not, if other facts did not render the explanation invalid) by assuming that the child possessed simply the power of multiplying mentally, with extreme rapidity and correctness, but in the ordinary way.[1] But the next test removes at once all possibility of explaining his work as done in the ordinary manner. He was asked what number, multiplied by itself, gave 106,929, and he answered 327, *before the original number could be written down.* This was wonderful. But he next achieved a more wonderful feat still, judging his work by the

[1] The account does not say whether he gave the figures successively from right to left or from left to right. If he began at the left, ordinary multiplication would not explain his success; for no one, however skilful, could multiply a number of thirteen or fourteen figures by a number of one figure so rapidly as to begin at once to name the left-hand digits.

usual rules. He was asked what number, multiplied twice into itself, gave 268,336,125—in other words, to find the cube root of that array of digits; *with equal facility and promptness* he replied, 645. Now, anyone acquainted with the process for finding the cube root—even the most convenient form of the process, as presented by Colenso and others—knows that the cube root of a number of nine digits could not be correctly determined, with pen and paper, in less than three or four minutes, if so soon. If the computor had so perfect a power of calculating mentally that he could proceed as safely as though writing down every step, and as rapidly with each line as Colburn himself in the simple processes before described, he would yet need half a minute at least to get the correct result. This, too, would imply such a power of mentally picturing sets of figures that, even if it explained Colburn's work, it would still be altogether marvellous, if not utterly inexplicable. We know, however, that Colburn was not following ordinary rules, but a method peculiar to himself. In point of fact, he was so entirely ignorant of the usual modes of procedure, that he could not perform on paper a simple sum in multiplication or division.

Let us proceed to further instances of his remarkable power of calculation.

On being asked how many minutes there are in 48 years, he answered, before the question could be written down, 25,228,800; which is correct, if the extra days

for leap years are left out of account. He immediately after gave the correct number of seconds.

We come next, however, to results which appear much more surprising to the mathematician than any of the above, because they relate to questions for which mathematicians have not been able to provide any systematic method of procedure whatever. He was asked to name two numbers which, multiplied together, would give the number 247,483, and he immediately named 941 and 263, which are the only two numbers satisfying the condition. The same problem being set with respect to the number 171,395, he named the following pairs of numbers: 5 and 34,279; 7 and 24,485; 59 and 2,905: 83 and 2,065; 35 and 4,897; 295 and 581; and, lastly, 413 and 415. (I presume, as Mr. Baily gives the pairs in this order, that they were so announced by Colburn. The point is of some importance in considering the explanation of the boy's mental procedure.) The next feat was a wonderful one. He was asked to name a number which will divide 36,083 exactly, and he immediately replied that there is no such number; in other words, he recognised this number as what is called a *prime* number, or a number only divisible by itself and by unity, just as readily and quickly as most people would recognise 17, 19, or 23 as such a number, and a great deal more quickly than probably nine persons out of ten would recognise 53 or 59 as such.

Now, if a mathematician were set such a problem,

he would have no other resource than to deal with it by
direct trial.   Of course he would not try every number
from 1 upwards to 36,083.   He would know that, if
the number can be divided at all, it must be divisible
by a number less than 190 : for any greater divisor
would go, exactly, some smaller number of times into
36,083 ; and that smaller number would itself be a
divisor.   He would see that the number is not even,
and therefore cannot be divided by 2, 4, 6, or any even
number.   The number is not divisible by 3 ; for, ac-
cording to a well-known rule, if it were, the sum of its
digits would be so divisible ; therefore he would dismiss
3, 9, 15, and all numbers divisible by 3 not already
dismissed.   So with 5 (for the number does not end
with a 5) ; so with 7, by trial : 11, 13, 17, and so on.
But he would have to try many numbers of two and
three figures by actual division before he had completed
his proof that 36,083 has no divisors.   Probably (for
I must confess I have not tried) he would require
about a quarter of an hour of calculation before he
could be confident that 36,083 is a prime number.
Here however was a child, eight years old, who, to all
appearance, completed the work immediately the num-
ber was proposed !

· The next feat was of the same nature, but very
much more difficult ; indeed, it taxed the young calcu-
lator's powers more than any other feat he accomplished.
Fermat, a mathematician who gave great attention to
the theory of numbers, had been led, by reasoning
which need not here be considered, to the conclusion

that, if the number 2 be multiplied into itself 31 times
(that is, raised to the thirty-second power), and 1
added, the result will be a prime number. The result-
ing number is 4,294,967,297. The celebrated mathe-
matician Euler succeeded, however, after a great deal
of labour (and, if the truth must be told, after a great
waste of time), in showing that this number is divisible
by 641. The number was submitted to Zerah Colburn,
who was of course not informed of Euler's prior dealings
with the problem, and, *after the lapse of some weeks*,
the child-calculator discovered the result which the
veteran Swiss mathematician had achieved with much
greater labour.

Before proceeding to inquire how Colburn achieved
these wonders, we must consider what was learned about
his processes. He was not very communicative,—
doubtless because the faculty he possessed was not
accompanied by commensurate clearness of ideas in
other matters. In fact, we might as reasonably expect
to find a child of eight years competent to explain pro-
cesses of calculating, however easily effected, as to find
him able to explain how he breathed or spoke. One
answer which he made to a mathematician who pressed
him more than others to describe his method was clever,
though the mathematician was certainly not to be
ridiculed for trying to get the true explanation of Col-
burn's seemingly mysterious powers—'God,' said the
child, 'put these things into my head, and I cannot
put them into yours.'

Some things, however, he explained as far as he

could.   He did not seem able to multiply together, at
once, two numbers which *both* contained many figures.
He would decompose one or other into its factors, and
work with these separately.   For instance, being asked
to multiply 4,395 by itself, he treated 4,395 as the
product of 293 and 15, first multiplying 293 by itself,
and then multiplying the product twice by 15.   On
being asked to multiply 999,999 by itself, he treated
it, in like manner, as the product of 37,037 and 27,
getting the correct result.   In this case, probably, a
mathematician would have got the start of him, by
treating 999,999 as a million less one, whence, by a
well-known rule, its square is a million millions and
one, less two millions, or 999,998,000,001.   'On
being interrogated,' proceeds the account, 'as to the
method by which he obtained these results, the boy
constantly declared that he did not know *how* the
answers came into his mind.   In the act of multiplying
two numbers together, and in the raising of powers, it
was evident (alike from the facts just stated and from
the motion of his lips) that *some* operation was going
forward in his mind, yet the operation could not, from
the readiness with which the answers were furnished,
have been at all allied to the usual modes of procedure.'

Baily, after discussing the remarkable feats of Zerah
Colburn, expressed the opinion that they indicate the
existence of properties of numbers, as yet undiscovered,
somewhat analogous to those on which the system of
logarithms is based.   'And if,' says Carpenter (quoting
Baily), 'as Zerah grew older, he had become able to

make-known to others the methods by which his results were obtained, a real advance in knowledge might have been looked for. But it seems to have been the case with him, as with George Bidder and other 'calculating boys,' that with the *general* culture of the mind this *special* power faded away.'

With all respect for a mathematician so competent to judge on such matters as Francis Baily, I must say his explanation seems to me altogether insufficient. So far from the properties of logarithms illustrating the feats of Zerah Colburn, they illustrate the power of mathematical development in precisely the opposite direction. The system of logarithms enabled the calculator to obtain results more quickly than of old, *not* by the *more* active exercise of his own powers of calculation, but by employing results accumulated by the labours of others. Its great advantage, and the quality which causes every mathematician to be grateful to the memory of Neper of Merchistoun, resides in the fact that, by taking advantage of a well-known property of numbers, tables of moderate dimensions serve a great number of purposes which by any ordinary plan of tabulation would require several volumes of great size. If it were possible for a calculator to use as readily a set of tables equal in bulk to five volumes of the 'London Directory' as he now uses a book of logarithms, and if such volumes could be as easily and as cheaply produced, tables much more labour-saving than the books of logarithms could be constructed. But of course such sets of volumes would be practically

useless if they could be produced, and it would be impossible either to find calculators to form the tables or printers and publishers to bring them out. Now, of all processes by which mathematical calculation can be carried out, no two can be more unlike than mental arithmetic on the one hand, and the use of tables, of whatever kind, on the other. Neper invented his system to reduce as far as possible the mental effort in calculation, making the calculator employ results collected by others; young Colburn's success depended on mental readiness, and he was so far from using the results obtained by others, that he did not even know the ordinary methods of arithmetic. A man of Neper's way of thinking would be the last to trust to mental calculation ; whereas, if Colburn had retained his skill until he had acquired power to explain his method, he would have been the last to think of such a help to calculation as a table of logarithms. Neper strongly urged the advantage of aids to calculation ; Colburn would scarcely have been able to imagine their necessity.

Nor is it at all likely—we could even say it is not possible—that properties of numbers exist through the knowledge of which what Colburn did could be commonly done. The mathematicians who have dealt with the theory of numbers have been too numerous and too skilful, and have worked too diligently in their field of research, to overlook such properties, if they existed. Besides, it is scarcely reasonable to suppose that a child who had but lately learned the nature of

numbers, and was altogether unaquainted with the
ordinary properties, should have intuitively recognised
abstruser properties. A more natural explanation must
surely exist, if we consider the matter attentively.

It happens that I am able, from my own ex-
perience, to advance an explanation which accords
well with the facts, and especially with the circum-
stance that calculating boys usually lose their ex-
ceptional power of rapid reckoning when they are
instructed in and taught to practise the ordinary
methods; for I used formerly to possess, though in a
slight degree only, a power of finding divisors, pro-
ducts, and so on, which—*unlike ordinary skill in cal-
culation*—required only to be expanded to effect what
Colburn effected. It was, in point of fact, simply
the power of picturing a number (not the written
number, but so many ' things '), and changes in the
number, corresponding to division or multiplication
as the case might be. Thus the number 24 would
be presented as two columns of dots each containing
ten, and one column containing four on the right
of the columns of ten. If this number were to be
multiplied by three, all that was necessary was to
picture three set of dots like that just described; then
to conceive the imperfect columns brought together
on the right, giving six columns of ten and ' three
columns each of four dots; and these three gave at once
(by heaping them up properly) another column of ten
with two over: in all seven columns of ten and one
column of two,—that is, seventy-two. This takes long

in writing, but, as pictured in the mind's eye, the
three sets representing 24 formed themselves into the
single set representing 72 in the twinkling of an eye
(if the mind's eye can be imagined twinkling). The
process for division was not exactly the reverse of
that for multiplication. Thus, 72 being pictured as
seven columns of ten and one of two, to divide it by
3, the first six columns of ten were pictured as giving
twenty sets of three horizontal dots ; the next column
of ten gave three vertical triplets, counted from the
top ; and then the remaining dot at the bottom, with
the other two in the imperfect column gave another
triplet, or twenty-four triplets in all. These triplets
could all be *seen* as it were ; and the only mental
calculation properly so called consisted in counting
them, which of course was easy, twenty of them being
as it were already numbered.

It is easy, with practice, for anyone of average
powers to conceive in this way numbers up to several
hundreds, and to imagine such processes of change as
I have described in a simple case. Of course this
fact does not in one sense explain Colburn's feats
with much larger numbers. For instance, I should
have been as helpless to deal with the numbers
Colburn attacked, as anyone who had never adopted
the particular method of dealing with numbers de-
scribed above. But there is this distinction between
that method and the ordinary method. No conceiv-
able amount of acquired skill in carrying out the
ordinary arithmetical processes mentally could account

for Colburn's feats; but the power required for the
other method needs only to be possessed to an enhanced
degree to enable the calculator to accomplish feats of
the kind./ It will be observed that when a number
has been mentally pictured as a set of columns—so
many units, tens, hundreds, thousands, and so on—
the mind can proceed to picture this array of dots
forming themselves into rank and file, so many wide
and so many deep, with so many over when a com-
plete rectangular phalanx is not .formed. If in any
such pictured arrangement there are none thus left
over, then the number in each rank is one divisor, and
that in each file is another. If the mental sergeant
after conceiving the army set two deep, three deep, four
deep, and so on, until rank is exceeded by file, finds no
single case where there are *none* left over, then the
number thus dealt with has no divisors. Again, if two
equal multipliers are wanted to make up a number, or
technically, if the square root of a number is wanted,
the mind, after picturing the number, forms it into
square,—the equal number in rank and file being the
required square root. Conversely, if a number is given
to be multiplied by itself, the mind pictures a square
army of dots with that number in rank and file, and
then forms the army into columns of tens, hundreds,
thousands, &c. Finding the cube roots depends on
the same power of picturing a number of dots, only
instead of picturing them as arranged on a flat surface
like an army, they were probably conceived as set up
within cube-shaped spaces. This would not be necessary

in cubing numbers, or multiplying any number twice into itself; but in the reverse process it would be the readiest method.   Still, quite possibly, the mental process actually followed by Colburn, when a number was given him whose cube root was required, may have simply corresponded to the rapid array of the army representing the number into a number of squares each having as many in rank and file as there were squares.   Thus, suppose the number 64 (which to persons of average capacity for conceiving a number of points or dots would correspond to a large number submitted to Colburn), then the mind would successively picture this number as presented by two ranks of thirty-two and four ranks of sixteen, stopping at the last arrangement, because perceiving that these *four* ranks could be divided into *four* squares, each of *four*. The required cube root then is *four*.

But it may be argued that, admitting this explanation, the marvellous nature of Colburn's feat is in no way diminished.   For, to minds of average power, the faculty of picturing the enormous arrays which the explanation requires is something altogether inconceivable.   I am not concerned to make the feats of Colburn, Bidder, and others, appear less marvellous than they are usually considered.   They are unquestionably altogether amazing.   But the point to which I would direct attention is that they involve marvellous developments of a faculty we all possess to *some* degree, and do not depend on hitherto undiscovered properties of numbers.   It will be · seen that,

according to the explanation I have given, it is not some advanced and recondite property of numbers that is in question, but the mental development of the most elementary method of. dealing with numbers,— by actually picturing them. Apart from the mathematical grounds which exist for preferring this explanation to the other, it obviously seems 'more reasonable to infer that a faculty showing itself at an early stage of mental development (for every remarkable calculator has begun young, and most of them have entirely lost the faculty as they advanced towards manhood) must depend on the simplest principles of numbers, not on principles so abstruse as hitherto to have escaped detection even by the most advanced inquirers into numerical relations.

But the opinion the reader may form on such an explanation as I have here advanced will in part depend, no doubt, on the question whether independent evidence exists to show that the mind can form perfect pictures of a great number of objects, and conceive processes of change to take place, following these processes as confidently as though they took place under the eyes or were effected by the hands of the person conceiving them. It appears to me that there are few apter illustrations of this faculty than we find in the power which some chess-players possess of conducting several games simultaneously without seeing the board. It seems a sufficiently wonderful feat to play a single game without the board, and more wonderful perhaps to a good chess-player than to those little familiar with

the game.   We find, indeed, that for a long time after
the game was invented the attempt was never made to
play without boards.   Glanvill, in his 'Vanity of
Dogmatizing' (1661), talks of a 'blind man manag-
ing a game at chess' much as one would speak of a
blind man using a telescope,—as a thing absurd on the
face of it.   He was a chess-player, too; and one
would suppose he had at times thought over the games
he had formerly played, and thus learned to some
degree how a game can be mentally followed.   But,
as I have said, the feat of blindfold chess-playing
is even more wonderful to a chess-player who does
not possess the power of calling up before the mind a
complete picture of board and men at any stage of a
game, than it is to one unfamiliar with chess.   For the
player knows how varied the resources of the game
commonly are at each stage, how the choice of any
move from amongst several which are available depends
on consequences calculated seven or eight moves deep
(not for the selected move alone, but for each of the
rejected moves).   Now if the blindfold player reasoned
out each move, by *considering* the scope and influence
of each piece, arguing mentally, for instance, that such
and such a piece having been moved to such and such
a square commands such and such other squares, or can
be brought in so many moves to some desired position,
or must be guarded by such and such steps from other
pieces, it would be simply impossible for him to con-
duct a game, or at least to complete one within any
reasonable time.   Yet, strangely enough, many chess-
players suppose that it is a feat of this kind which the

blind-fold player accomplishes. And necessarily the marvel, already great, becomes almost incredible when we recollect that three, ten, twelve, nay, if I remember rightly,[1] in one case twenty, blindfold games have been conducted simultaneously by one player.

The real meaning of the feat is understood, however, when we notice that some of the strongest chess-players have been unable to play blindfold, precisely as some of the greatest mathematicians have been unable to deal mentally with any but the very simplest problems. Philidor and La Bourdonnais could both play without seeing the board, but McDonnell, St. Amant, and Staunton never accomplished the feat (at least in any recorded *partie*). Harrwitz could play blindfold; his rival Horwitz could not. At the present day Blackburne and Zukertort can play ten or twelve games blindfold, but several of the strongest chess-players living do not, I believe, possess the power. We must, therefore, find an explanation which shall not require the blindfold player to be superior in chess-strength to the player who is unable to carry on a contest without seeing the board. The explanation is simple. The blindfold player is able to picture to himself the board and men, at any stage of a game, and thus plays mentally with as much ease and confi-

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[1] We believe Paulsen accomplished on one occasion the feat of playing twenty games simultaneously without seeing the board. We know certainly that Morphy, the stronger player of the two (and probably the strongest chess-player ever known), admitted that Paulsen could conduct more blindfold games simultaneously than himself, yet Morphy often played twelve blindfold games at once.

dence as if he had the board before him. If he is
conducting a dozen blindfold games simultaneously, his
method is the same. I am unable to say, however,
whether he pictures all the games at once as though
the boards were ranged before him, or calls up a
mental picture of each board with the men properly
placed for that game as its turn comes round. Probably,
in most cases, the latter is the method adopted. It
matters little for my argument which manner of con-
ceiving the boards and men[1] is preferred. In either
case, we perceive that the mind must have a complete
record of a great number of objects, and a power of
conceiving changes of position amongst these objects,
strictly analogous to that by which we have endeavoured
to explain the feats of Zerah Colburn. When a blind-
fold chess-player (or rather a player without board, for
the blindfolding is merely nominal) is conducting
twelve games, he has, either in one mental image or
available for successive study, twelve boards, each with
64 squares, or 768 squares to be separately recognised,
with in all (at starting) 384 men. At every step he
has to select between several alternative moves, each
admitting of several alternative replies, each reply sug-
gesting various lines of play, so that the total number

[1] In speaking of several games played simultaneously, I have
no experience of my own to guide me. In my youth, I used often
to play a single game without board, and I can still conduct a game in
that way, though not so readily as of old, a break of nearly twenty
years in chess practice having had the effect of diminishing the com-
pleteness of the mental image of board and men. It does not appear
to me that I should find more difficulty in playing two or three
games in this way than in playing one, though of course my play
would be slower.

of moves to be considered increases in geometrical proportion. To consider each position effectively, he must conceive the various steps of each line of play as actually taking place before him. To do this for ten or twelve games, against good players, surrounded by spectators who expect each game to progress without undue delay (so that he must play ten or twelve times as rapidly as his opponents) requires unquestionably a power of mental calculation rivalling in degree that shown in the feats of Colburn, Bidder, and others, though altogether different in kind. In fact it has been remarked by Todhunter, the mathematician, that skill in chess is a quality not unlike mathematics, as a test of mental power, though with this important difference, that mathematics properly employed are useful in science, whereas the skill shown by the chess-player can afford no results corresponding to the labour acquired in attaining such skill. Of mere mathematical skill, apart from its useful application, Todhunter says well, that it is not so highly to be esteemed as the practice at Cambridge suggests. 'It seems at least partially to resemble the chess-playing power which we find marvellously developed in some persons. The feats which we see or know to be performed by adepts at this game are very striking, but the utility of them may be doubted, whether we regard the chess-player as an end to himself or his country.' This view of the resemblance between mathematical feats and feats of chess-playing has been—independently—enunciated also by Professor Atkinson, of Massachusetts Institute of Technology, who speaks of mathematical problems

by themselves, and divorced from their connection with
the physical sciences, as 'hardly rising in dignity or
educational value above the game of chess.'[1]

There can be no doubt that some persons possess
the power of forming mental pictures so perfect as to
serve all the purposes of objective realities—that is, to
admit (as in the case I have supposed to be illustrated
by the feats of mental calculators and chess-players) of
processes which may be called mental manipulation.
Most of us have experienced the existence of this
faculty in dreams. For instance, we dream of reading
a book and the mental conception of the book is so
perfect that, as it were, we turn leaf after leaf, finding
each page perfectly presented—paper, type, arrange-

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[1] It is rather singular that a chess-player of note, Herr Kling, has
given to a treatise on the game the title 'Chess-Euclid.' Ernest
Morphy, uncle of Paul Morphy, has written a work entitled the 'Logic
of Chess.' We find in a sketch of P. Morphy's doings in Europe, in
1858, a passage at once indicating his natural aptitude for the game
and its quasi-mathematical character. 'In answer to a gentleman in
Paris as to whether he had not studied many works on chess, I heard
him state,' says the writer of the sketch, 'that no author had been of
much value to him, and that he was astonished at finding various posi-
tions and solutions given as novel—certain moves producing certain
results, &c.,' *for that he had made the same deductions himself as neces-
sary consequences.* In like manner, Newton demonstrated in his own
mind the problems of Euclid, the enunciations only being given, 'and
I can think of no more suitable epithet for Morphy than to call him the
"Newton of Chess."' The last sentence, however, is absurd. I may
add that it was not Newton, but, if I remember rightly, Thos.
Chalmers, who is said to have supplied the demonstrations of Euclid's
propositions when the enunciations only had been given him (Newton
said that the propositions appeared to him self-evident). Several well-
known mathematicians have been skilful chess-players; and Anderssen,
who was victor in the chess tourneys of 1851 and 1862, is Professor of
Mathematics in the University of Breslau.

ment, &c., all pictured by a process of unconscious cerebration, precisely corresponding to the conscious action of the mind which I have assumed in my explanation of Colburn's mastery over a certain class of arithmetical problems. The same faculty is exercised by the artist who draws either from memory or by a sort of creative talent which enables him to conceive suitable forms or attitudes, and copy them as though the conceptions were realities. Dr. Richardson, in an interesting essay on hallucinations, mentions a singular illustration of this faculty in the case of Wm. Blake. This artist once 'produced three hundred portraits from his own hand in one year.' When asked on what this peculiar power of rapid work depended, he answered 'that when a sitter came to him, he looked at him attentively for half-an-hour, sketching from time to time on the canvas; then he put away the canvas and took another sitter. When he wished to resume the first portrait, he said, "I took the man, and put him in the chair, were I saw him as distinctly as if he had been before me in his own proper person. When I looked at the chair, I saw the man."' [1] It may be well

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[1] I may mention here the story that Garrick once sat for another man's portrait. It need scarcely be said perhaps that the story is not strictly true. It was, however, based on a fact. The likeness of Fielding forming the frontispiece to Murphy's edition of Fielding's novels was drawn by Hogarth from memory after the novelist's death. 'Being unable,' Sturz says, 'to call to mind some peculiar expression about the mouth, Garrick came to his aid by imitating it.' In reality, the power of imitating some expression of another's countenance is a faculty well worth considering. It would be a matter of curious inquiry (in the scientific sense) to discuss why some persons are able to

to mention that the exercise of this faculty is fraught
with danger in some cases. Blake, after a while, began
to lose the power of distinguishing 'between the real
and imaginary sitters, so that' (the *sequitur* is not
quite manifest, however) 'he became actually insane,
and remained in an asylum for thirty years. Then his
mind was restored to him, and he resumed the use of
the pencil; but the old evil threatened to return, and
he once more forsook his art, soon afterwards to die.'

It may perhaps appear to the reader that this case,
however remarkable in itself, does not prove the possi-
bility of conceiving with perfect distinctness other
objects than were retained in the memory, and there-
fore is not sufficient to explain the mental feats before
considered. But there are cases not less remarkably
illustrating the distinctness of the mental vision, where
the objects conceived were certainly not called up by
an act of memory. Thus Talma the tragedian could at
will picture a crowded audience as so many skeletons,
each perfect in every detail corresponding to the atti-
tude of the person thus metamorphosed. This case is
the more remarkable that usually the exercise of the
bodily eye interferes with that of the mind's eye.
Talma was not only able to picture the theatre as full
of skeletons, but they became so real in appearance,
that he acted as though they were his auditors and
critics: and Hyacinthe Langlois tells us that Talma's

imitate almost any expression, while others with equal command of
feature, when attempting to represent a particular expression, produce
an expression which is totally different, though perhaps equally charac-
teristic.

acting was rendered more intensely effective by the imagined presence of these singular spectators.

However, I am not concerned now to inquire into the nature of hallucinatory manifestations, and I mention these stories only as indicating the power which the mind possesses of calling up images of objects not merely remembered, but formed, as it were, by the mind's own act. It may be presumed that every complete image thus formed is produced by combining objects already known and remembered. In the process of mental arithmetic above described, the mental 'counters' appeared (in the writer's case) as whitish spots on a dark ground. In mental chess-playing there must be great diversity. It would be interesting to ascertain from Morphy, Blackburne, and the rest, what sort of mental boards and pieces they employ; for such masters of the art of blindfold play must see well-defined pictures,—chess-boards and men which they could describe and could get made for them.[1] My own mental chess-board is ill-finished about the edges, and the men have shadowy supports which, as it were, elude me when

[1] Thus a Cerebral Museum might present as curiosities the board and men with which Morphy played such and such a game blindfold; the set of eight boards and men used by Blackburne in mental play on such and such an occasion. We have all heard of the sword exhibited as the one Balaam had in his hand when the ass addressed him (hallucinatory manifestation, no doubt), and how, when it was pointed out that he only wished for such a sword, the relic was described as 'the very sword he wished for;' but, if I may say so without spoiling a good story, supposing Balaam had a clear mental idea of the sword with which he would have liked to kill that obnoxious ass, a sword constructed accordingly would be an interesting object to the student of mental phenomena.

I try to determine their character. Probably every reader of these lines has met with a similar difficulty in attempting to determine the exact nature of a mental image. For instance, the newspaper account of an accident states, let us say, that 'several persons who were standing near' when an accident happened did so and so; on reading this you immediately have a mental image of several persons, but they are shadowy beings, and if you try to determine precisely what they are like, they become still more shadowy or vanish altogether. (But probably no two persons have the same experience in such matters.)[1]

It may be questioned whether some remarkable feats of memory may not be explained by the power of forming mental pictures, though of course the power of recalling the sequences of sound must oftener be that on which the remembrance of long series of notes and numbers depend.

It is to be noted, in considering feats of memory relating to written or spoken words, that apart from artificial aids to memory, there are at least three ways in which memory may act :—

We may remember the facts, and may thus often re-

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[1] The ' person ' who makes his (or her) appearance in this case must have had, I suppose, some definite origin, but he (or she) is not easily identified. The mental 'bystander' is a different being altogether. I have never been able to satisfy myself whether the 'person' came out of books or pictures known to me in childhood, or had some real original. I half incline to think that *my* 'person' is an imperfect mental image of a woman, a sort of nurse-housekeeper, who was the first person I knew much of outside the family circle. Something in the aspect of the mental person, when momentarily called up with unusual clearness, reminds me of that monitress of my childhood.

call the words also, especially if these are particularly appropriate or striking. Indeed, there are some passages which could hardly be recalled without the appropriate words, simply because no other words or arrangement of words would present the same ideas so well. For instance, the soliloquy in *Macbeth,* beginning ' If it were done, when 'tis done, etc.,' might be recalled word for word by one who had carefully noted the sequence of ideas, and also of course (as part of this way of remembering) the use of particular words and images. Every sentence brings in the next, and very little effort of memory is required to recall such peculiarities of expression as 'trammel up the consequence,' 'with his surcease success,' 'the be-all and the end-all here,' and so on. Some parts of the soliloquy in *Hamlet,* beginning ' To be, or not to be,' might even be recalled by the incongruity of the images.[1] It is not probable that any of the more remarkable feats of memory recorded can

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[1] It does not seem to us at all clear that the critics (from Voltaire downwards) who have abused Shakespeare for making Hamlet talk of taking 'arms against a sea of troubles,' are better justified than a moralist would be who should object to the reasoning of Iago. It is of course possible that Shakespeare wrote a careless line in this noble soliloquy. Jonson tells us that Shakespeare, in extemporising on one occasion in the character of Julius Cæsar, brought out the amazing line, ' Cæsar never doth wrong but with good cause.' Extemporising and writing, however, are different matters ; and it seems on the whole safer to consider that Shakespeare had some idea what he was writing when he created the soliloquies of Hamlet. The point is to some degree connected with the question—about which so much has been written to so little purpose—whether Shakespeare intended to present Hamlet as really insane, at least from the time of the interview with the Ghost. Though this seems untenable, yet in all the later soliloquies (which really determine the point) there is manifest evidence of mental disturbance, whereas only a certain perturbation of spirit is shown in the soliloquy of the second scene. Shakespeare's purpose

be explained by the exercise of this method, which may be called *reasoning memory*, though this kind of memory is undoubtedly the most valuable, and perhaps the only kind necessarily indicating mental power, in the usual sense of the words.

Secondly, we may remember a passage by the mere sequence of words or sounds without reference (or at least without special reference) to the sense. This method may be called *verbal* or, preferably, *syllabic memory*. We are all of us more or less familiar with this kind of memory, even though we may not often, or perhaps ever, adopt this particular way of learning passages by heart. A passage learned otherwise, but often repeated after being learned, comes to be repeated in this manner.[1] But the point to be noted is that

seems so clear that one wonders how anyone can mistake it. Hamlet's pretended madness was first thought of, where Hamlet says,

> ' How strange or odd soe'er I bear myself, .
> As I, perchance, hereafter shall think meet
> To put an antic disposition on.'

This was too soon after the interview with the Ghost to be regarded as a deliberately adopted plan. Manifestly it was intended at first merely to account to his friends for ' the wild and whirling words' he had just used : he is beginning to recover himself, and perceives how strange his behaviour must seem to Horatio and Marcellus. As the scene closes, his wildness has given place to settled despair. We have said that the soliloquies decide the question of Shakespeare's real intention —if a true poet can be said to weigh such matters. (We know how Richter said, ' A poet who doubts whether a character shall say this or that, to the devil with him.' The soliloquy following the interview with the Ghost is specially decisive of the state of Hamlet's mind *then*. Who but one half-crazed for the moment would have thought of jotting down a note about the smiling of villains, just after he had heard of his father's murder, and from his father's ghost ?

[1] It is indeed singular how retentive this kind of memory is. In

the power of learning syllabically — so to describe
this method—is probably the true interpretation of
the feats of memory which are commonly regarded
as so astonishing,—as indeed they are, though not
in the sense in which they are usually apprehended.
I have already referred in these pages[1] to a sugges-
tion by Wendell Holmes, that this kind of memory
might be regarded as a useful dynamometer, ' which
may yet find its place in education.' It appears
to me that the faculty is not more closely associated
with true mental power than the faculty of recalling
a tune which has been heard once or seldom. It is
indeed a faculty of precisely the same kind; though

saying this, I do not refer to the remembrance of familiar passages
often repeated, for in reality such instances prove nothing ; but there
are cases where a passage learned in childhood, and not repeated for
many successive years, is found not only to be retained as a passage
having such and such a meaning, but syllabically, even with imperfec-
tions belonging to the time when it was learned. The following
instance seems worth mentioning:—When about eight years old,
and long before I began to learn Greek under proper tuition, I was
led by childish ambition to con a Greek grammar belonging to a
schoolfellow. My first step was naturally to learn the alphabet, and it
so chanced that I took the ordinary pronunciation of every letter ex-
cept ' Chi,' the ' ch' in which I pronounced as in ' child.' The Greek
grammar was soon dropped; and of course when the study of the lan-
guage was entered upon, the usual pronounciation of ' Chi ' was indi-
cated by the teacher. But though, perhaps, the Greek alphabet was
not half a dozen times repeated in the old manner from that time for
twenty or thirty onwards, yet to this day, if I were to repeat the
Greek alphabet while my thoughts were occupied with other matters,
the wrong ' chi' would come out. It is the same with several Latin
words learned in France when I was still younger, and still pronounced
in the French way, unless by an effort the English or the more correct
Continental pronunciation be adopted.
  [1] See the essay on the Growth and Decay of Mind.

it may well be that a person possessing one of these
faculties may not always possess the other. If we
rightly consider the case of a person who, having
heard a long-continued sequence of notes forming an
air or tune, is able to repeat the sequence correctly,
we shall find it at least as remarkable as a case like
that mentioned by Pepys, of a person who could repeat
sixty unconnected words, or of the Indian who could
repeat a long passage in Greek or Hebrew after it had
been once recited, though ignorant of either language.
It happens curiously enough that Paul Morphy, the
chess-player, possesses in an unusual (though not
actually phenomenal) degree the power referred to. In
the work recording his achievements in chess, for
instance, we find this passage :—' In the evening we
went to the Opéra-Comique, and witnessed a very un-
satisfactory performance of *La Part du Diable*.
Morphy has a great love for music, and his memory for
any air he has once heard is astonishing. . . . *La Part
dū Diable* was a new opera, yet Morphy, after leaving
the theatre, hummed over many of the airs to me,
which he had just heard for the first time, with
astonishing precision.' Of course the noteworthy point,
here, is that Morphy is not a musician.

The third way in which a passage may be re-
membered is by the aid of a mental picture of the
words forming the passage. After what we have seen
of the achievements of Colburn, Morphy, and others,
by means of the power of forming mental pictures, it
need not surprise us if some persons can call up a

mental picture of complete pages of letter-press, so
that page after page may be mentally turned, as it
were, and the words in them read off precisely as
though the mental book were an objective reality.
Nor is it at all improbable that this method of re-
membering series of words may explain some re-
markable cases in which recited words have been re-
peated. We manifestly cannot explain in this way
such a feat as the Indian's above mentioned, or any
case in which uneducated persons have repeated long
series of words; for it is impossible for persons who
cannot read to form mental pictures of words. But the
case mentioned by Pepys can be explained in this way,
and some of the feats of Pepys's memory-man can
scarcely be explained otherwise. A man with a ready
imagination can picture a word as printed so soon as it
is uttered, and if several successive words are repeated,
he will picture the series as clearly as though a page con-
taining them were before his eyes. If he has a mental
faculty corresponding to what Gustave Doré calls
'collodion in the eye,' the picture thus formed can
be recalled at any time, and the whole series of words
repeated. I have said that some feats can scarcely
be otherwise explained. Pepys tells us that the prodigy
he describes could repeat a series of recited words
backwards almost or quite as readily as forwards.
Here, then, there was no syllabic repetition. How-
ever perfectly we may recall a series of words by
syllabic memory, it is not easy to repeat the words
backwards, as anyone (not troubled with the fears of

being reputed a practiser of the evil art) can in a moment test by trying to repeat the Lord's Prayer backwards. Much less could anyone repeat backwards a series of words only just learned by syllabic memory. No doubt the man whose feats so astonished Pepys possessed the power of picturing each word as a printed word as soon as it was uttered ; and having thus formed in his mind's eye a complete picture of a long series of words, he could repeat them as readily backwards as though he were reading a series of words backwards from a book.[1]

Mental pictures may not only be formed in this way, but mental processes corresponding to particular actions may be carried on ; and whatever the explanation may be, it is certain that skill in such

[1] It is probable that some of my readers may not be aware of the use of this faculty of mental picturing, to recall forgotten words. For there are many who possess the faculty but, never exercising it, remain ignorant of its existence. (Once recognised, the faculty may be greatly strengthened by use.) As a simple illustration of the useful exercise of this power, the writer cites a case which occurred a few days ago to himself. He was about to address a letter to a friend in the country, when he found that the most necessary part of the address had escaped his recollection. The name of the person, the name of the house, and the name of a large town near which was the park by which the house was situated were recalled, but the name of the park was forgotten, and as the writer was in a hurry to catch a particular post, of course the more the name was mentally hunted after, the less chance there was of recalling it. But his friend used note-paper with the address engraved in full upon it ; and though none of his letters were at hand to show the address, it occurred to the writer that, on his forming a mental picture of the remembered part of the address, the forgotten word would appear in its proper and well-remembered place. This happened the moment the attempt was made. I would prefer not to say whether the letter was in time for post. It *might* have been, which is all the reader need care to know.

actions may be acquired by such mere mental exercise. In some cases this is in no way remarkable. For instance, we can easily understand that when a passage is repeated mentally the power of repeating it aloud may be acquired or increased. But it is different when the action to be acquired is strictly mechanical. It seems worth noticing, by those who make a special study of the brain and its powers, that a series of movements may be, as it were, *practised* mentally. For we are in the habit of regarding practised movements as acquired by associating certain mental processes with the actual performance of corresponding bodily actions, and it is not easy to explain, according to any known theory of cerebral action, how the association of mental processes with actions only conceived mentally can give skill resembling that derived from actual practice. Consider, for instance, one who is learning a piece of music for the piano, not having as yet acquired the art of immediately manipulating any indicated movement. We can understand how, by actually practising a difficult movement in the piece, such a learner can acquire the art of rendering it easily and effectively; for the theory of the brain tells us how certain muscles learn to respond in a particular way, and in proper time and sequence, to the messages conveyed by the visual nerves to the brain. But it is strange that when those muscles have not actually been exercised, but merely the idea of their use excited, the learner should yet acquire the art of using them in the required way.

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Even more remarkable, however, is the fact that dexterity in particular processes is often inherited; nay not only so, but sometimes, as it were, developed and intensified in the inheriting. We have hitherto not referred to the theory that some instances of wonderful mental power are to be explained by the doctrine of heredity. In fact, the instances we have been dealing with do not, so far as we know, illustrate that doctrine. Morphy's uncle is a strong chess-player, but not a fanatic for chess. Morphy's skill was shown indeed at an early age (when only twelve years old he won two games out of three, and drew the third, against Herr Löwenthal), but that point of itself is not sufficient to indicate *directly* inherited ability. Of Zerah Colburn's ancestry we know nothing. In passing, we may mention a circumstance which possibly may be connected with his phenomenal skill in elementary arithmetic. There is, or was, a family of Colburns, mentioned in the *American Popular Science Monthly* for November 1873, in which 'the parents for four generations transmitted to the children what is called sex-digitism—*i.e.*, hands and feet with six fingers each;' and considering the important part which the fingers play in the arithmetic of childhood, one can imagine that the young Colburns must have acquired unusual mastery over numbers. Not that six fingers on each hand would be at all convenient in numeration; but on the contrary, because learning the elements of arithmetic by the usual decimal system would be rendered more difficult, and the learner

compelled to master arithmetical relations more thoroughly. However, we do not know even that Zerah Colburn was a member of this many-fingered family—and certainly he was not duodecimally fingered himself. It is noteworthy that, notwithstanding the antiquity of the science of numeration, the examples of skill hereditarily transmitted are much fewer than we might expect, especially when it is remembered that the sons of a mathematician have a better chance than others of receiving good mathematical training, and therefore a mathematical bias. Except the Bernouillis, and perhaps the Cassinis, we can recall no families in which mathematical talent has seemed to be in any sense hereditary.

It is otherwise with music, and perhaps with painting.[1] Heredity shows itself more markedly, it would seem, in the arts than in the sciences. Taking music, we find some remarkable instances. The Bach family, which took its rise in 1550 and became extinct in 1800, presents an unbroken series of musicians for nearly two centuries of that interval. The head of the family was Veit Bach, a baker of Presburg, and his two sons were the first of the family who were musicians

[1] In Titian's family we find the names of nine painters. The Caraccis, Teniers, Vanderveldes, Van Ostades, Hondekoeters, and others, will occur at once. Special methods of drawing and painting seem also to be inherited, not merely imitated. In the present writer's family the construction of large drawings in pen and ink (so as to have the appearance of engravings) has been a favourite employment of leisure time; and in the last three generations, certainly the taste was not imitated, circumstances having prevented this. Thus, in the writer's own case, the taste first showed itself several years after the death of the parent from whom it was inherited.

by profession.   The descendants literally 'overran
Thuringia, Saxony, and Franconia,' says Papillon.
'They were all organists, church singers, or what is
called in Germany "city musicians."   When they
became too numerous to live all together, and the
members of this family were scattered abroad, they
resolved to meet once a year, on a stated day,
with a view to keep up a sort of patriarchal bond of
union.   This custom was kept up until nearly the
middle of the eighteenth century, and oftentimes more
than 100 persons bearing the name of Bach, men,
women, and children, were to be seen assembled.   In
the family are reckoned twenty-nine eminent musicians,
and twenty-eight of a lower grade.'   Rossini's family
played music at fairs; Beethoven's father and grand-
father were musicians; Mozart's father was Second
Capellmeister to the Prince-Bishop of Salzburg.

  We are prepared then to find, in the theory of
transmitted habits, the explanation of the wonderful
musical powers of Mozart, with some account of which
(from his life by Holmes) we must close this note,
already drawn far beyond the limits proposed when we
began :—' When Mozart's sister, then seven years old,
was learning to play on the clavier (the early form of
the piano), Mozart, then three years old, was a con-
stant attendant on her lessons; and already showed, by
his fondness for striking thirds, and pleasing his ear by
the discovery of other harmonious intervals, a lively
interest in music.   At four, he could always retain in
memory the brilliant solos in the concertos which he

heard; and now his father began, half in sport, to give him lessons. The musical faculty seems to have been intuitive in him; for in learning to play he learned to compose at the same time; his own nature discovering to him some important secrets in melody, rhythm, symmetry, and the art of setting a bass. To learn a minuet, he required half an hour; for a longer piece, an hour; and having once mastered them, he played them with perfect neatness and in exact time. His progress was so great that at four years of age or earlier, he composed little pieces, which his father wrote down for him.' Later, 'In music, he astonished his teacher, not so much by an avidity for information, as by the impossibility of telling him anything that he did not know before. At the age of six Mozart knew the effect of sounds as represented by notes, and had overcome the difficulty of composing unaided by an instrument. Having commenced composition with recourse to the clavier, his powers in mental music constantly increased, and he soon imagined effects of which the original type existed only in his brain.'

But in some respects perhaps the most remarkable circumstance related in this life of Mozart is the following. When he was only seven years old, his father took him to see an organ with pedals. 'To amuse ourselves,' says the father, in a letter to a friend, 'I explained the pedals to Wolfgang. He began immediately, *stante pede*, to try them, pushed the stool back, and preluded standing and treading the bass, and really as if he had practised many months. Everyone was

astonished; this is a new gift of God, which many only attain after much labour.'

To sum up,—we perceive that the human mind is capable of forming pictures of processes, by following which mentally calculations of considerable complexity may be carried out, and other useful results obtained; we see that the mind can so perfectly picture some processes as to help in actually training the body by mere mental exercise; and lastly, we note that such powers, and even the accumulated results of long years of experience, may in some cases be transmitted hereditarily. In these facts we may recognise interesting evidence respecting the possible future development of the human mind.

(From the ' Cornhill Magazine ' for August 1875.)

AUTOMATIC CHESS AND CARD PLAYING.

WHATEVER opinion we may form on the question whether men are automata, we must admit that men make automata achieve remarkable feats as mechanical ingenuity increases. The machines employed in manufactures, which must be regarded as automatic workmen, grow so wonderfully towards perfection that they have been described (almost truly) as actually thinking for their employers. Dr. Anderson, for example, a civil engineer and a practical man, speaks thus of two orders of instruments, one order inferior to the other but still

wonderful in its way :—Instruments of the former kind
' may be said to work by a sort of blind routine; they
have only a single idea imparted to them which they
reiterate; but they have not the faculty of thinking for
themselves. If any difficulty arises in the course of
their working, they are at a loss what to do, and not
unfrequently break their hearts over the dilemma,'
(poor things). But those of the superior order, ' not
only have ideas embodied in them,' like the former,
' but, in addition, they have what we may almost call
a reasoning faculty. They have the capacity of putting
several ideas together, then running up the existing
conditions, and arriving at a practical decision in a
fraction of a second, a mental process which would
occupy a learned philosopher for hours, even if [he
were] furnished with all the facts of the case.' ' There
are other tools,' he proceeds, ' which are provided with
a nervous system, which pervades their mechanism,
whereby if any disorder of their normal condition
occurs they instantly communicate the fact to a sort of
brain, and stop of their own accord. Other tools
perform the most difficult mathematical calculations,
and are capable of printing the result, so that no error
may occur in copying.'

We have not, however, any intention of considering
here the various processes used in machinery, or the
manner in which the reasoning, controlling, and (what
Paley thought incredible) the reproductive powers of
machinery may be illustrated. Nor, indeed, would any
description of the contrivances used in automata, be in

place in these pages. We propose simply to consider in a general way some of the questions suggested by the feats of so-called automata, like Kemplen's chess-player, the chess-player at the Crystal Palace, and Mr. Maskelyne's Psycho.

So far as we know, no really automatic player of games of skill has yet been constructed. Babbage devised a machine for playing the lively game called ' noughts and crosses,' which, however can hardly be called a game of skill. But it is noteworthy that Babbage believed in the possibility of making a really automatic chess-player. His reasoning was sound, so far as abstract possibility is concerned; though he certainly did not succeed in showing how the feat was practically to be accomplished. The argument for the theoretical possibility may be thus presented:—A chessboard has sixty-four squares and there are thirty-two men. Hence, the actual number of arrangements of the men is limited, and would be so even if each man might stand on any square, and if there could be any disproportion between the two forces in point of number. But, in reality the number of possible positions is considerably reduced by the peculiarities belonging to the nature of the game. Thus, a pawn never stands either on the first or eighth rank, while many positions in which the pieces might be set up cannot possibly be brought about in actual play. For example, a position in which a bishop is anywhere except at his own square, while the two pawns which prevent his leaving that square are unmoved, is impossible in actual play; and

there are many other positions which cannot result in
a real game.[1] Even, however, with all such deductions,
the number of possible positions must be counted by
millions.   Now, suppose we take any position whatever,
and that it is white's turn to play.   There must be
some move which is better for him than any other, or
—to be more exact—which is not surpassed in strength
by any other move.   And it must be possible, by play-
ing a sufficient number of games from that position, to
find out what that move is.   We say possible, not
meaning practicable.   Thousands of games might be
played from that position; still if thousands of players
were set to work to go through these games *seriatim*,
the consequences of every possible line of play from
that position could be determined with certainty.   Not,
however, to make the task of the inventor more arduous
than it need be, let us simply suppose that any such
selected position is submitted to the analysis of twenty
players of the first class.   Then doubtless either the
very best or at least a very effective move would be

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[1] Except when the arrangement of the pieces at starting is altered
—a plan adopted by Mongredien, formerly President of the St. George's
Club, to equalise players learned and unlearned in the book openings.
Again, games are sometimes played under unusual conditions—though
more rarely now than in former times.   To the true lover of the royal
game such arrangements are as far removed from real chess, as the
game described by Noailles in Tennyson's *Queen Mary*—

> Strange game of chess! a King
> That with her own pawns plays against a Queen,
> Whose play is all to find herself a King.
> . . . . a knight
> That with an ass's, not a horse's, head
> Skips every way.

found for white. Suppose this done in succession for all the possible positions. (The task of finding a winning move would be in many cases exceedingly easy.) This done, and the results carefully recorded, the task of the mechanician begins. What *he* has to do is to provide that on the formation of any given position by the move of a black piece, mechanism should be started which would cause the automaton to make the proper move, already assigned, for white. This Babbage pronounced to be mechanically feasible. We need not here consider precisely how it might be done; but the principles on which the move of a black piece might be made to cause one particular set of movements can easily be indicated. Suppose each piece to stand on a base of a particular shape which would fit in only one way into a corresponding depressed portion of each square of the chess-board; suppose further each different piece, (queen, bishop, rook, and so on,) to have a definite set of projections underneath, so that when a piece is set down into the depression of a square, a definite set of springs would be pressed, which, when lifted, would be released. Then, when a black piece was lifted, releasing certain springs, the concealed machinery would assume the condition corresponding to the position *without* that piece; but when the black piece was set down in a new place by the player, certain springs would be touched, while the rest of the machinery would be in a particular condition. Thus, the machinery would be actuated in a definite manner and

a definite move would be the result.[1] This move might
either be simply the removal of a white piece from one
position to another; or the replacement of a black
piece (or pawn) by a white piece; or the movement of
a white pawn to the eighth row (either simply or by a
capture) and its replacement by some white piece—or,
in addition, check might be given to the black king,
and the automaton might indicate this by some move-
ment of his head or hands. All this could readily be
effected by machinery. And, equally well, movements
might be arranged to indicate that a false move had
been made by the automaton's opponent, either by
moving a piece incorrectly, or by leaving the black
king in check.

So far as concerns the mechanism required to effect
these movements, we need not hesitate to accept
Babbage's assurance that there would be no insuperable
or very serious difficulty. The real difficulty in the
construction of an automatic chess-player resides in the
preliminary investigation of the chess positions. These
are so numerous, and in a large number of·cases the
selection of the best move is so difficult, that it would
be hopeless to attempt the task, even if some great
advantage to the whole human race promised to reward
such labours. And since, in reality, they would be·

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[1] These springs would be equally touched by the same piece moved.
to that square from any other position; but either they would not,
when so moved, actuate the same mechanism (the other internal arrange-
ments being differently situated at the moment), or else, though they
might actuate the same mechanism, it would operate on other mecha-
nism differently placed, and so produce different effects.

almost absolutely profitless, it is quite certain that
such a task will never be undertaken. We may say,
indeed, that the labour would be absolutely profitless,
since the only persons who could conceivably gain by
an achievement of the kind would be the proprietors of
the machinery, and it is certain that the cost would
enormously exceed any possible returns they could
obtain by exhibiting the automaton.

We may take it for granted then, whenever we see
an automaton chess-player ready to encounter all
comers—that is, not merely playing through a series of
set games, either with the exhibitor or a confederate—
that there is a concealed player directing the automa-
ton's play. The concealed player need not necessarily
be within the figure—though in all the automatic
chess-players yet made this has been the case. If he
is placed anywhere so that he can see the board, he
might by suitably arranged mechanism, cause the
automaton to make the proper move. However, the
player has hitherto been concealed within the figure.
In Kemplen's, the board stood above the space where
Maelzel was concealed while the automaton was actually
playing. When the interior was exposed to view, by
opening a series of doors, Maelzel changed his posi-
tion from one part to another, until on closing the
last door he took up his position for playing. Under-
neath the board were attached sixty-four threads, one
for each square, each carrying a small iron ball.
Within each piece was a strong magnet, so that the ball
under any square on which a piece stood was attracted

to the under side of the board. So soon as a piece was moved, the ball under it, ceasing to be attracted, was left to hang by its thread; and when the piece was put down in a new place, the ball which had been hanging there immediately jumped up and adhered to the underside of the board. Or if a piece was taken, then, when it was removed the ball under it fell, and when the capturing piece took the place of the captured, the ball flew up again. Maelzel had a small board in his lap on which he repeated the move of the automaton's opponent as indicated by the movements of the balls. Having decided on his reply he communicated the proper movements to the automaton. And so the game proceeded, with little more difficulty to Maelzel than if he were playing in the ordinary way; for by continual practice he was able to make the moves on his own board, both for his opponent and for himself, and to set the machinery in motion, with scarce a second's loss of time. We do not, of course, mean that he played in a second —for when he was encountered by strong players he was often obliged to deliberate over his moves—but that he did not take more than a second or two over the merely mechanical part of his work.[1]

As regards Maelzel's power as a chess-player, it would not be easy to form an opinion, though fifty of

[1] Kemplen's automaton was destroyed by fire in Philadelphia, and the nature of the mechanism was never fully explained. We believe, however, that the above account of the arrangement for indicating the moves to Maelzel was either given by Kemplen himself, or admitted by him to be correct.

his games have been preserved. He was certainly not a player of the first force in set encounters, though he vanquished some very good players. Of course, when the automaton was exhibited quick play was expected on both sides. Those who visit an exhibition of the sort are seldom much interested in chess-playing itself, and could certainly not endure with patience the slow play of a chess match, even as chess matches have been conducted since the time limit was introduced.[1] Maelzel himself played with astonishing rapidity; and his opponents usually felt bound to emulate him in this

[1] According to a recent arrangement adopted for chess tourneys, each player is allowed an hour for twenty moves, or some other number agreed upon. Thus, if he plays several moves somewhat rapidly he may give a quarter of an hour or twenty minutes to a single move at some crisis of the game. Occasionally also, twenty minutes' grace, or the like, is allowed once in the game, over and above the time allowance for the other moves. But before the time-limit was introduced, players would sometimes give an hour and more to a single move. Kolish, a skilful German, once gave two hours to a move in a game which (we are glad to be able to add) he lost, though he won the match of which it formed part. In the amusing little work, the *Chess Player's Annual* (the only published volume, so far as we know), edited by Professor Tomlinson of King's College, the following passage occurs in the description of a game played by Tomlinson and three friends in consultation against Herr Löwenthal, Howard Staunton looking on. ' L. considered for some time, and Beta asked Mr. Staunton what was the longest time he had ever to wait for a move. ' The longest time,' said S., ' I ever had to wait was in playing a match with a man who wore out everybody—seconds, spectators, and myself. We had been playing many hours, and were left alone, when he coolly said, ' I'm a poor man and cannot afford to lose this match. I must sit you out.' That being the case, and no witnesses present, I had nothing to do but to give up the match, and write him a cheque for the money.'" In the same paper, mention is made of a fourteen hours' sitting at chess. But after all, this is nothing to a case recently recorded in the *Westminster Gazette*, in which a sitting at whist lasted four days.

respect, or at least to move much more rapidly than any player would think of doing in a set encounter. The natural result usually followed in the case of Maelzel's opponents—they made oversights—while he himself was by long practice able to play very rapidly without making mistakes, or at least without making such palpable blunders as his opponents.

It has been stated that the automaton chess-player at the Crystal Palace is only a copy of Kemplen's chess-player. But anyone who remembers the older automaton, or who has read a correct description of it, will perceive that the present figure is different in several essential respects. Kemplen's automaton played with his left hand, a peculiarity only noticed by the ingenious contriver of the machine, when it was too late to modify the arrangement. The automaton at the Crystal Palace plays with the right hand. But the most important difference is in the position of the chess-board. The board of Kemplen's figure rested on a table (really a chest), and the movements were recorded directly under the board, in the manner already explained. The chess-board of the Crystal Palace automaton stands on a single upright pillar, of small cross section. The mover of the present automaton is, beyond doubt, concealed within the figure, not in some adjacent place where he could see the board ; and the various parts of the interior are shown, as in the case of Kemplen's automaton. But several parts are thrown open at once, so that as a good number of visitors may be in the room at the same time, and there is no re-

striction on their movements while the interior is being exhibited, it appears impossible that a living person can be concealed, at least a person old enough to play a good game at chess. But the human frame can be concealed in a much smaller space than is usually supposed, if the space is specially arranged for the purpose. It is not true, as was stated by the manager of a rival automaton, that the concealed player retreats under the floor itself of the room, before the doors are thrown open ; for since that statement was made the figure has been raised above the floor. But it is certain that during the play the player is within the figure. Immediately after the last door is closed the frame of the figure vibrates perceptibly, precisely as one would expect it to vibrate as a concealed player squeezed himself, as it were, into the proper position for observing the play of opponents and guiding the motions of the figure.

It seems to us probable, that at the same moment a portion of the machinery, which during the inspection had seemed to occupy the chest of the figure, may be shifted, so that room is found there for the head of the concealed player. It is tolerably certain that the player can see the board itself (which was not the case with Maelzel). He can hear also what is said by the visitors, even in low tones. We tested this on one occasion. A friend, who is a tolerably good player, was conducting a game, and from time to time we suggested a move or two. To this the automaton made no objection, though consultation is forbidden (of which

at the time I was not aware), and he might have
indicated objection by shaking his head, as when a
false move is made. But it happened that at one
stage of the game I perceived a line of play by which
a noteworthy advantage could have been gained, and
whispered accordingly to my friend, giving the series
of moves in a low voice; so low, in fact, that my
friend did not catch the suggestion perfectly. So
soon as the complete series of moves had been in-
dicated, the automaton's head wagged disapproval,
doubtless because the line suggested would have been
fatal to the automaton's game. That the suggestion
was sound, appeared soon, for our friend adopted
another move, and in a move or two was compelled
to resign; when, by the gracious permission of the
automaton, the position was restored, and on ex-
amination it was found that a piece must have been
captured by the proposed line of play. It is clear,
then, that the concealed player, who had been in-
different when indifferent suggestions were made, had
caught the drift of the proposed line of play, and
objected accordingly. He made no movement until
the whole scheme had been indicated. No doubt ex-
perience has taught him that in most cases such
suggestions are likely rather to help his game; and in
the present case the series of moves began with a
sacrifice which, if unsound, would have given him
the game at once. The very instant he perceived that
it was sound, he set the automaton's head shaking.

The automaton plays a fair game, though not a

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very strong one. I have played several games with him, usually with the result that, after securing a winning game, some egregious blunder has brought destruction upon me. But this can scarcely fail to happen, even with practised players, when twenty or thirty moves are played on both sides in between five and ten minutes; and I am by no means a practised player, not having played perhaps so many as fifty games in all, during the last twenty years. I have, however, twice beaten the automaton, and twice drawn games with him, so that I have had opportunities of examining his play under varying conditions. (Those who have not seen him beaten may be interested to know that his manner of acknowledging defeat is by removing his king from the board.) He plays endgames better than the openings, though with inexperienced players his management of the openings is effective. It really is worth while for one who is learning chess to try a few games with the automaton, if only to observe how quickly he breaks through the defences around a castled king. He plays a game which usually gives him an attack of this kind. When he has the first move he opens with what is called the Giuoco Piano (or quiet game), adopting for fourth and fifth moves, usually, the advance of queen's pawn one square and the castling of his king. For the sixth move he plays what is often regarded as a weak move, advancing his king's rook's pawn one square, which with him has a double object; first it prevents his king's knight from being pinned, and next it gives

that piece a square to move to, which is very seldom
so occupied by good players, yet affords a retreat whence
the knight can be very effectually brought out at the
right moment. In the meantime his opponent has
usually brought out his king's knight and castled,
without the precaution of providing against the pinning
of the knight, a process which the automaton immediately
attends to ; after which, if his opponent is a weak player
the automaton makes very short work of him. On
the other hand, if he is second player, the automaton
will accept none of the familiar gambits which lead
to the well-trodden book openings, except one which
cannot safely be refused (called the Scotch gambit).
Tried by ourselves twice with the familiar king's
gambit, the automaton has both times declined it,
and we have seen the same occur with others. The
brilliant Evans gambit, which has now been analyzed
twenty moves deep, he avoids by simply declining to
bring out his king's bishop at the third move, play-
ing instead his king's rook's pawn one square. This
is his weak point, for unquestionably the first player
can then so continue the game that the automaton is
practically two moves behind, and exposed to very
strong assault. However, usually the opening player
falls into a line of operations in which the automaton's
weak move becomes a serviceable one ; and often the
game pursues much the same course, after a while, as
when the automaton has the first move.

It will be understood that there is a little sameness
in the openings. Indeed, I have seen identically the

same moves played in two games on both sides up to the twelfth or thereabouts—certainly without collusion.[1] Yet chess is a game so full of resources that no one need fear that a series of games with the automaton will want the charm of variety. Besides, it is open to any player who wishes to take the automaton off the beaten track, to adopt some out-of-the-way opening, and this whether the automaton play first or second move. (For each game sixpence is charged).

It would appear that this wooden-headed gentleman

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[1] There are some openings at chess where the moves on both sides fall in so naturally, as it were, that it is quite common for the first ten or twelve moves to be repeated even by players who, though strong, are not booked-learned in chess. There is one particular form, indeed, of the Scotch gambit which has led several times to the same game right through, mate occurring to the second player soon after the twentieth move. The game is given in full in the *Westminster Papers*, for Oct. 1874, with the following letter, over the initials of a well-known amateur:—' Nothing is more common than to hear it said that the same game of chess, *i.e.*, the same sequence of moves, has never been played more than once. There is, however, a curious exception to this rule, with which many of your readers may not be acquainted. Some six months ago at the Divan, I was playing a Scotch gambit, and was able to repeat move after move, a game which I had perused not long before in a well-known work, the *Neueste Theorie und Praxis* (a game between Klierforth and Schliemann). The only remarkable thing here was the unswerving fidelity with which my opponent followed the model, even to the extent of suffering the rather pretty mate. When the game was finished, I, of course, explained my source of inspiration; and Herren Steinitz and Zukertort, to whom the curiosity was shown, told us that each had previously played the game, move for move, on at least one occasion. Only the other day I discovered another edition, in an old volume of the *Leipsiger Schachzeitung* (vol. for 1870, p. 229), and, therefore, being of opinion that this variation has already had more than its due share of success, I shall beg you to set up a light-house near the dangerous rock, for the benefit of your weaker chess brethren.' For the further benefit of those who may examine the game, we may mention that the second player's trouble arises from his seventh move.

is not above studying the books, for on a chair behind
the figure one may usually see a book which looks like
Staunton's familiar Hand-book. On the occasion of my
last visit, it so chanced that no visitors were present
(the theatre was open, and *Our American Cousin* was
being performed). After paying my sixpence, I had
to wait a few minutes before the door was opened,
during which time, doubtless, the player concealed him-
self within the figure. On entering, I noticed that
the hand-book was turned down on its open face, and,
so far as I could judge, it was opened about where the
automaton's favourite opening is dealt with by Staunton.
Yet I must admit that there is very little book-work
in the automaton's game.

With regard to the whist-playing of Psycho, I am
not able to speak from experience in actual play. I
fancied at my first visit that the invitation addressed
to the audience to join in the game, was like a Spaniard's
invitation to his guests to regard his house and all that
is in it as their own. At least the gentlemen who took
part in the game on that occasion exhibited a coolness
of demeanour on the platform which one would not
expect from persons who had volunteered merely lest
the performance should come to a stand-still.

But it so chanced that at my second visit a very
eminent man of science, president of one of the chief
learned societies, went on the stage to examine the
figure, and eventually took a hand at whist. He most
assuredly was not a confederate, and it is certain, there-
fore, that the automaton plays a *bonâ-fide* game. In

passing, I may remark that Psycho's play is not very
profound, nor even always sound.[1]

The natural idea in the case of an automaton like
Psycho is that there is a player concealed within the
figure.   The figure and box are certainly large enough
to give room for a small boy.   Nor does the prodding
which the figure receives from Mr. Maskelyne prove
that there is no boy so concealed, for there is room
between the rod and the face of the lower enclosure for
a boy's legs.   I suppose the boy to be in a sitting
but somewhat askew posture, with his knees where the
legs of the figure appear.   I find from the dimensions
of the figure and box, as indicated in the photograph
(Mr. Maskelyne's height in the picture affording a
sufficient scale of measurement), that there is room for
a boy about four feet in height, and rather thin, but
not remarkably so.   It is not necessary to suppose that
the boy plays as good a game at whist as Psycho seems
to do, for his play in all the rounds but one or two may
be directed by Mr. Maskelyne, pre-arranged signals
indicating the *number* of the card to be played, that is,
its position on the arc in front of Psycho.   Mr. Maske-
lyne appears certainly to see each card as he inserts it
in the arc, though he disclaims all knowledge of the

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[1] In one recorded game we find Psycho afflicted by the prevailing
epidemic of the Blue Peter—signalling for trumps without reason.   I
have not noticed whether he has adopted the device of playing lowest
but one in leading from five in suit.   His discarding struck my inex-
perienced mind as rather wild.   But as his partner was satisfied, I
assume the play was right.

hand. With a little practice, a very moderate memory could retain the remembrance of thirteen cards.

I may note in passing, that beyond all reasonable doubt the card tricks are performed by Mr. Maskelyne, and the work of the automaton is limited to the extraction of the card, properly placed for the purpose in the small box into which the exhibitor carefully bestows the pack. And this may serve to guide us in forming an opinion about the whist-play and the numerical calculations. But it would serve no useful purpose to inquire in what particular way mechanical effects are brought about in a case like this, where there can be no such inspection as would really help to determine the *modus operandi.* Indeed, when well-devised conjuring tricks are shown, one can usually conceive many ways in which the effects might be obtained, so far as observed facts are concerned; and the odds are heavy against the right method being hit upon, so that usually the performer can assert (and *prove*) any suggested explanation to be incorrect.[1]

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[1] Sometimes the explanation of a trick which seems unusually difficult is, in reality, unusually simple. Take, for instance, such a card trick as the following, which, so far as I know, is not in the books :—A pack of cards is handed to a person, who is asked to select any card, remove it, and return the pack, which is then handed to (say) two other persons in the same way. After this the conjuror asks the first person to take the pack again, and place the selected card anywhere in it. On receiving back the pack the conjuror proceeds, in any way he thinks likely to be effective, to produce the card thus taken and returned, presently showing that he also knows the two other drawn cards, which were not returned. Supposing the three persons not to be confederates, what, at a first view, can be more perplexing than this? The cards

It will be more interesting to inquire whether a figure might not be made to do all that Psycho does without any person being concealed within it, and without any material connection with the person guiding its movements. Let it be distinctly understood that in what follows I am describing a method certainly *not* employed for working any automaton card-player or chess-player ever yet made.

I pass over such contrivances as the use of a powerful magnet, or the imperceptible tilting of a portion of the floor on which a figure stands, whereby gravity may be caused to act on a concealed arm suitably loaded and adjusted (as in the case of certain clocks). In fact, as I said at the outset, any consideration of mechanical details or the like would here be quite out of place. What I propose is, simply to show how an unseen apparatus might be made to direct

can neither have been 'forced' on the persons drawing them, nor can the conjuror have in any way noted where the returned card was placed (if he could, this would not help him to a knowledge of the other two cards), for the pack was out of his hands when the cards were taken and when one of them was returned. Yet nothing can be simpler than *one* way in which the trick can be performed, nor easier to the practised card-conjuror. A fine line drawn diagonally down one of the long sides of the pack affords a sharp-eyed person the ready means of detecting at a glance any card returned to the pack in a new position, and also the former position of any other cards which have been removed without being returned. Knowing so much, the conjuror, talking a little about card-tricks (using the pack to illustrate what he says), can find abundant opportunities to bring the returned card (the marked edge of which he knows by its displacement) to the top or bottom of the pack, or to produce it in any desired way, and to see each of the cards next preceding the other removed ones, whence, knowing the arrangement of the pack, he knows the removed cards themselves.

the motions of a figure,—a whist-playing figure, let us say,—placed at a considerable distance from it.

The motive power I would select then (among many that might be thought of), would be dark heat, such as resides in a mass of metal heated just short of luminosity. Or a hollow globe filled with hot water would serve equally well. Either would retain a sufficient quantity of heat for a much longer time than the exhibition would last. This source of heat could be placed above the figure or on the same level, at any convenient distance. A curved mirror must be placed so as to reflect the obscure heat convergingly towards the figure. This mirror must be capable of easy adjustment, so that the heat-focus can be shifted at will to any one of a set of determinate points of the figure. The mirror and heated globe could readily be placed where the audience, or even anyone on the stage, could not see them. For it is only necessary that they should be within the visual range of an eye occupying the place of a part of the figure itself, and the line from this part to the globe might readily be so situated that no person could place himself where he could look along it. For example, the part of the figure on which the rays were converged might be seven feet above the stage; or again, the heated globe might be placed either beyond the audience or beyond the whist-table, and then persons on the stage might reasonably be requested to avoid getting in the way of the audience, or between the figure and the whist-table, the only positions whence they could see the globe and mirror. But in

reality there would be no occasion that the globe and mirror should even be in *sight* of the figure (that is, placed so that to an eye situated where the figure is they could be seen). For heat can pass where light cannot pass, and a screen of black glass or smoked quartz covering the niche or other receptacle wherein the globe and mirror were placed would cut off very little of the heat, while effectually hiding the globe and mirror from view.[1]

Having now a motive force and the power of directing its action on different parts of the figure, we might adopt a variety of contrivances for making this action produce any desired movements. The mere expansive effect of the heat would suffice to start delicate machinery, whereby the well-poised arm of the figure could be brought round to a particular position, the hand then nipping a card, lifting it, and placing it down as Psycho does. The construction of mechanism merely to effect these movements would of course be a very simple matter. What perplexes those who see automata at work is not the invention of mechanism for producing the movements, but to imagine how the mechanism is started and stopped without being touched. No one acquainted with the resources of mechanism would wonder, for instance, if Psycho were seen to play the right card when a particular knob was touched. The source of wonder is the action of the figure without apparent guidance.

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[1] The globe might even be concealed by the focussing apparatus, substituting for the mirror a lens of black glass or smoked quartz.

Now the heat from the concealed globe could be converged by the mirror on any desired point, and would as effectually set machinery in motion as though a knob at that point had been pressed. If the mere expansive action of the heat were insufficient, or in other words if the machinery were not of sufficient delicacy to be so set in motion, then the knobs could be the faces of small thermopiles, such as lecturers use to display the effects of small changes of temperature. A surface no larger than a sixpence, and easily made quite undistinguishable from the neighbouring surface, would suffice to generate an electric current and start the machinery, with less heat than is generated by moving a finger a few inches along a piece of wood. Thirteen such surfaces would be as thirteen knobs for setting the figure to play any one of thirteen cards. And in a much smaller figure than Psycho (even without the box he stands on) thirty or forty distinct operations might easily be provided for by mechanism, and directed by the motion of the concealed mirror, deflecting the heat on any one of thirty or forty corresponding points of the figure's surface. If the heat globe were large and the mirror good, the sensitive surfaces might be very small, and act entirely by expansion through heat. In this case the figure might be a mere doll in size. If, on the other hand, thermopiles were used, a figure nearly as large as Pyscho would be required, because of the room required for suitably conveying the electric currents; but in this case the globe and mirror might be very small. Of

course the figure would have to be placed in a very
particular position, and the movements of the mirror
carefully arranged beforehand. Thus, let us suppose
that when the figure was set on its pedestal, and the
mirror in its mean position, the heat was focussed on a
non-sensitive spot in the middle of the sensitive points
(arranged in rows). Suppose now that the concealed
operator, who might very well be so placed as to see
both the card-table and the cards held by the figure,
or who might be guided by a pre-arranged system of
signalling (as in the ordinary *clairvoyance* trick[1]) per-
ceives that the point he must act upon to cause the
figure to play rightly is on the third row above and on
the fourth row to the right of this central point. Then
it might be arranged that by giving three turns to a
wheel causing the mirror to rotate so as to throw the
beam of heat upwards, and four turns to another
causing the mirror to rotate so as to throw the beam
towards the right, the heat-focus would be thrown
correctly, and in a second or two it would set the
right machinery in action.[2]

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[1] I do not refer here to such clairvoyance as Carpenter, Zerffi, and
others have dealt with, but to exhibitions of professed *clairvoyance*, in
which a child is made to describe articles shown to the exhibitor, who
conveys to the child, by the words he uses, a sufficient description of the
article (if it be any ordinary pocketable article).

[2] The operator may be a long way from the mirror thus worked,
seeing that there is no difficulty whatever in providing connecting
mechanism for directing a mirror from a distant point with the utmost
accuracy on any desired point. In astronomical observations, for ex-
ample, this is repeatedly accomplished. Nor, of course, would there be
any difficulty in concealing such mechanism, seeing that it is to act on a
concealed mirror.

A chess-playing figure might easily be constructed on a similar plan. The mechanical arrangements would of course be much more complex than for a whist-playing figure. Psycho, for instance, in playing whist, has only to be rotated on a vertical axis till the hand is over the right card, then the hand closes on the card, lifts, and drops it; and there are but thirteen cards. A chess-playing figure must not only be able to lift a piece from one square, setting him down on another (each piece according to its proper range of play), but must be able also to effect a capture, replacing a piece of the opponent's by a piece of his own.[1] But mechanical arrangements for this were provided in Kemplen's Automaton. (In that exhibited at the Crystal Palace I imagine the concealed player moves the pieces more directly, though a good deal of mechanism is shown.) What a concealed player could do to set such mechanism as in Kemplen's figure in motion could certainly be done by the 'globe and mirror;' and the automaton could then be much smaller, having to contain no concealed player.

But in reality no special interest attaches to the question whether another Psycho or another chess automaton might be made, the movements of which should be directed by a remote and invisible source of force. When such a scheme is mooted as that a daily

[1] Except in the case of taking a pawn 'in passing.' *Apropos des bottes*, here is a little puzzle for young chess-players. (It was designed by Zukertort, who based a neat chess-problem on it.) How can double check be given by moving a piece which does not itself give check to the adverse king?

journal should be set up in type in the provinces by a mechanism worked in London, all the so-called automata appear as mere toys. The point really worthy of notice is the command possessed by science over such matters, and the power which men of science possess, if they chose but to use it, to delude the world by wonder-working. If the object of science were to deceive men by mysterious exhibitions, all the contrivances of leger-demain, all the less ingenious devices of spiritualists and the like, would by comparison sink into utter insignificance. If we take only the wonders which science has accomplished (always openly), and imagine that men of science had agreed to hide their resources and methods from the world, how readily might a claim to supernatural powers have been established, or even pretended revelations promulgated! To take but a single instance. Who would dare to question what was said by men so favoured (apparently) by some power superior to natural laws, that they could communicate instantly with each other when thousands of miles apart? What profanity it would seem—supposing such men claimed to be messengers from a supreme being—to attempt to explain by natural laws the won-derful things they accomplished! We are fortunate in this, that the science of our time is outspoken. Not so many centuries ago something of the old spirit still remained, and great discoveries in science were guarded as carefully as the spiritualists in our time try to guard the contrivances by which they manage their decep-tions. We have, however, to pass much farther back

to reach a time when workers in science used their knowledge to delude the ignorant. I forbear from inquiring here how much of what was taught by the old astrologers and wonder-workers of Chaldæa and Egypt, to strengthen their position among an ignorant and superstitious people, has come down even to our own day, and not only in matters regarded as superstitions, but in some still held in reverence by many millions of men. I simply say, It is well for mankind that the men of science of our day (especially those most maligned for their outspokenness) are honest. Were they leagued to deceive the world by working wonders, their task would be an easy one. To undeceive the world would be much more difficult.
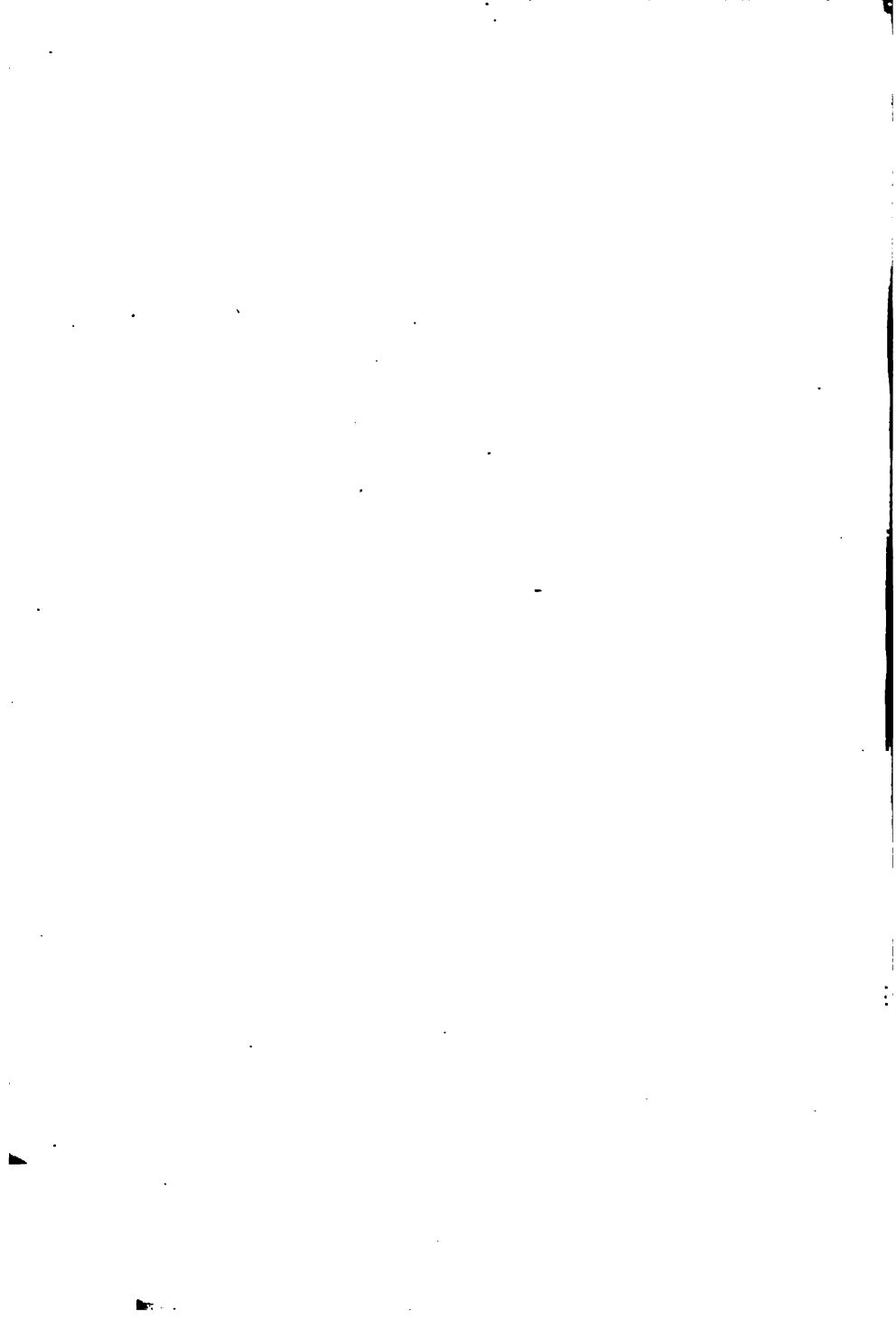
APPENDIX.

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MONEY FOR SCIENCE.

ABOUT four years ago a Commission was appointed, with
the Duke of Devonshire as Chairman, to inquire into the
methods available for extending scientific knowledge and
advancing scientific progress. The eighth and final report
of this Commission has just been published, so that we
now have before us the results of all the inquiries made by
the Commission. It will of course be understood that the
reports of the Commission do not represent scientific
opinion generally—only the opinion of the Commission
(sometimes of a majority only of its members) upon such
evidence as was brought before them. The evidence itself
was frequently conflicting, and, as usual in such cases,
many circumstances affected the selection of evidence, cer-
tain views being favoured, though of course not always to
the actual exclusion of others. Into points such as these,
however, I inquire further on. The general subject dealt
with by the Commission is one yearly growing in popular
interest, and some of the special results to which the
Commission have been led are well worth careful con-
sideration.

The first seven reports of the Commission related to
scientific instruction, and included an interesting survey of

the various institutions, endowed or unendowed, intended to aid scientific study. Perhaps the only point in all those reports which would be of interest to the general reader is the suggestion that the cost of scientific researches on a wide scale might be defrayed either wholly or in great part by the universities, from sums bequeathed to them in old times. The Commission cited evidence to show that the universities were not originally founded, as most persons imagine, for educational purposes, but for the advancement of knowledge. 'The collegiate foundations of the universities,' said one witness, 'were originally and fundamentally, though not absolutely and entirely, destined for that object.' And the report proceeds to say, justly enough, 'This object is certainly not less important in modern than [it was] in ancient society. In the middle ages, knowledge would altogether have perished if it had not been for such foundations, and it appears that now, from other causes, the pursuit of knowledge and of general scientific investigation is subject to very real dangers, though of another kind than those which then prevailed, and which make it very desirable to preserve any institution through which scientific discovery and the investigation of truth may be promoted.'

But it is in the last report that the question of obtaining large sums of money for promoting scientific research is first definitely entered upon. In fact, this last report may be regarded as an appeal to the Government for a goodly slice of the national funds. At present, about three hundred thousand pounds are expended annually by the nation on various scientific establishments. It is probable that if all the suggestions of the Royal Commission were carried out, the annual Government expenditure would rise to about ten times its present amount, and involve a very appreciable increase of taxation. So that we are all directly interested in the views which the Commission

have propounded. If these views are sound, and the proposed outlay would really be a most profitable investment, then the nation would gain an appreciable advantage by adopting the suggestions of the Commission; but if otherwise, the nation would suffer loss; apart from certain mis-chievous consequences not to be estimated by a pecuniary standard. If, as is more probable than either of these extreme views, the suggestions of the Commission are partly sound and partly questionable, it will be a matter well worth careful attention to divide these suggestions into their proper categories.

We may divide into three classes the objects to which it is proposed that Government should give assistance,—First, scientific researches by which the nation would, or might, gain some material advantage; secondly, scientific researches of philosophic interest only; and thirdly, the support of scientific workers, both by grants beyond the expenditure actually incurred in their investigations, and by the creation of well-salaried offices. The third of these divisions is, however, manifestly associated in some degree with the second.

Respecting scientific researches tending to advance the material interests of the nation, there will probably be little difference of opinion. Already such researches have been carried out at considerable yearly expense, and so far with advantage as to suggest that more money might well be devoted to them. There is an annual charge for the topographical survey of Great Britain, under control of the Treasury; another for the hydrological survey conducted by the Admiralty. The observatories at Greenwich and the Cape of Good Hope are under Admiralty control; while the Treasury maintains the observatories at Edinburgh and Dublin, and the botanical gardens at Kew, Dublin, and Edinburgh. The standard department under the Board of Trade, and the chemical department under the War Office,

involve a considerable annual expenditure. A sum of
10,000*l.* is annually voted also for meteorological observa-
tions.

Most of these departments of national scientific re-
search (so to call them) would certainly seem to require
increased grants. To take the meteorological observations
alone. The Commission in their last report express the
opinion ' that the operations of the Meteorological Office
have been attended with great advantage to science and to
the country ; ' but it would be difficult to show in what
that advantage consists. We have daily reports, and some
of our papers now give daily charts of the weather for the
twenty-four hours last *past.* But these are utterly useless
to the community, and the only value they could have with
men of science is precisely that which no man of science
has yet found in them—the possibility, namely, that from
them there might be deduced a trustworthy system of
weather-prediction. Occasionally a storm-warning is
issued. But no attempt is made to publish systematic
anticipations of approaching weather. Compare this with
the results which have been obtained in America, by the
judicious expenditure of a much larger annual sum. There
every day the daily papers (in all parts of the United
States) announce the probable weather of the coming day
for each division of the States, and these predictions are nine
times out of ten correct, and are not once in a hundred
times altogether wrong. As I have said, the annual cost of
the system is much greater in America. Some 50,000*l.* are
expended on the Meteorological Office. But surely it would
be better to spend 50,000*l.* for so useful a purpose than to
throw 10,000*l.* annually away as we now do. Though,
indeed, it does not seem quite clear why, if 50,000*l.* suffice
for the effective meteorological survey of the United States,
thirty times as large as the British Isles, a much smaller
sum might not suffice to supply corresponding results for

this country. The proper meteorological survey of all Europe ought to cost little more than that of the United States, seeing that Europe and the United States are not very unequal in extent. If England bore her fair share of such expenditure, our annual meteorological charges would be much less than 10,000*l.*

It appears, too, from the evidence brought before the Commission, that the scientific departments of the public service suffer from the want of scientific advice, which might be obtained at no very serious expense (compared at least with its value). Sir H. Rawlinson states that some blunders made in India, chiefly through this deficiency, ' may involve a loss of 200,000*l.* or 300,000*l.* to the British Government.' Capt. Douglas Galton expressed the opinion ' that there had been an enormous amount of money wasted upon inquiries into the best form of armour-plates, conducted by non-scientific persons.' The naval architect, Mr. Froude, C.E., considered that a vast saving might have been effected ' if there had existed proper laboratories in which experiments might have been carried out for the benefit of the Admiralty.' Sir W. Thomson gave as his opinion that if Government had enjoyed the advice of a scientific council, the *Captain* would never have been constructed on the ill-judged plan which resulted in her loss and the loss of more than five hundred lives. ' Very nearly 3,000,000*l.* of the nation's money is expended at Woolwich,' says the *Daily News,* ' expended judiciously according to official lights. But these lights do not appear to be brilliant or satisfactory ; for the frank avowal of the superintendent of machinery is " that we are groping in the dark in almost everything at present." The same eminent gentleman is confident that his steam-engines are not doing one-sixth of the work which they might be made to do if only a few qualified men were told off by Government to look into the point. The warden of the standards complains that he

can command no trustworthy scientific information with respect to trial-plates for coins ; and it is manifest that in not a few Government departments the same want is experienced.'

We cannot wonder, when we consider such points as these, at finding that there is almost complete consent among scientific men in favour of State aid for the forms. of scientific research I have been dealing with ; researches, namely, which have a national value, and not only promise to advance the material well-being of the people, but have (some of them) already done so in no inconsiderable degree. Nay, not only scientific men, but statesmen have naturally been influenced by such considerations. 'Lord Derby and Sir Stafford Northcote,' we are told, 'are almost as decided as the men of science themselves in their opinion that it may be legitimate and expedient to come to the aid of *researches which are beneficial to the whole community.'*

But when we pass from researches of this nature to researches having a merely philosophical interest, or which, though they may result in discoveries valuable to humanity, are not conducted with that direct aim, the case is considerably altered. Of course the thorough student of science has for his aim the discovery of truth, not the mere increase of the material wealth or power of man. But although this consideration will suffice to encourage researches not tending to ameliorate the condition of humanity or to increase a nation's store of wealth, it does not appear sufficient of itself to justify any large expenditure of the national money on such researches. The nation has a direct interest in one class of researches, while, as a *nation*, it takes no interest, and therefore in point of fact *has* no interest, in the other. The distinction should be carefully noted. Probably the number of persons in England who take an interest in the scientific principles of agriculture, meteorology, applied chemistry, and so forth, bears no greater proportion to the

population than the number who take an interest in the
physics of astronomy, in the exploration of the sea bottom,
in the existence or non-existence of an open sea round the
North Pole, or the like. But the whole population, though it
may *take* no interest in the former class of subjects, *has*, never-
theless, a very real interest in them ; and therefore scien-
tific discoveries in these subjects have a real value to the
nation.   Whereas the only value which discoveries about
the sun and planets, the sea-bottom, the North Pole, and so
on, can have, or be expected to have, resides in the interest
such discoveries excite in the philosophic mind ; so that for
those persons, at present the enormous majority of the
population, who take no interest in them, they have no
value whatever.   And though some of the national money
may very fairly be granted for the sake of that small but
select portion of the population which does take interest in
such matters, it becomes a serious question whether any
sums likely to increase taxation appreciably can in justice
be granted for researches in which the nation (as such)
takes little or no interest.   The average Englishman might
in that case very justly argue as Herbert Spencer has
imagined him to argue, addressing Government in some
such words as these : ' Your amiable anxiety for my welfare
shows itself in taking money *out of my pocket* to provide me
with various conveniences . . . . Out of my pocket, did I
say ?   Not always.   Sometimes out of the pocket of those
least able to afford it ; as when from poor "science
authors," who commonly lose by their works, you demand
gratis copies for your public libraries, that I and others,
who don't want to read them, may read them for nothing.
But these things you offer are things I do not ask.   I do not
want you to ascertain for me the nature of the sun's corona,
or to find a north-west passage, or to explore the bottom of
the sea.   Instead of doing what I want, you persist in doing
other things.   Instead of securing me the bread due to my

efforts, you give me a stone, a sculptured block from
Ephesus.' Where the sums devoted by Government to
scientific inquiries and expeditions are so small as not to
increase taxation appreciably, this sort of reasoning has of
course no great force; but it is of great weight as against
any wide scheme for the prosecution of scientific researches
not directly advancing the material interests of the nation.

It will probably be considered by most persons that the
course which has been actually adopted by Government for
many years past is a reasonable mean between the lavish
expenditure which those interested in science might desire,
and that absolute avoidance of expenditure on scientific
matters of mere philosophic interest which the majority of
the population would insist upon, could their votes be
taken.   What the Government has done has been to grant
assistance in such scientific operations only as could not be
carried out by private means.   Thus we have had eclipse
expeditions at a cost of several thousand pounds (indepen-
dent of the use of Government ships for transport), the late
transit expeditions at a cost of 15,000*l.* (a rather shabby
allowance, however, for this country), the voyage of the
Challenger, the expedition to the North Pole, and so on;
whilst doubtless we may expect that hereafter assistance of
the same kind will be afforded on a scale gradually increas-
ing as the scientific element of our population increases in
number and influence.

Unfortunately (as it seems to us) the report of the
Royal Commission suggests a much more considerable
expenditure on the part of Government for researches of
the class considered, and thus the just claims advanced in
the report seem likely to suffer by the intrusion of claims
which the country generally is by no means likely to ad-
mire.

To begin with : a proposition which might be enter-
tained reasonably enough if associated only with scientific

researches tending to increase the power and advance the material interests of the nation, assumes a totally different aspect in connection with the suggestion of lavish expenditure for the endowment of pure science. I refer to the suggested creation of a science ministry. It *might* be desirable (though even so limited a proposition is open to grave objections) to have a minister whose express duty should be the superintendence of the scientific departments of Government. And even the suggestion that ' in connection with and supplementary to this ministry there should be a sort of permanent scientific council whose advice the ministry might obtain ' (query, and follow ?) might be worth considering, if scientific matters of imperial moment only were to be dealt with. But a ministry having, as an important part of its duty, the control of large sums of money for researches of only philosophic interest, would certainly be objected to by the country at large, and the practical carrying out of this suggestion might excite a hostility to science and to scientific men which would most seriously injure the prospects of science in this country.

But let us consider some of the suggestions made by the Commission as to the actual scientific operations which, they think, might with advantage be subsidized by Government. I may premise that, although in nearly all of these suggestions relating to matters outside the scientific departments already considered, we find a reference to the possible advantages which might result from scientific discoveries, we must treat the suggested arrangements as relating only to matters of philosophic interest, because the nation cannot seriously be invited to take part in a mere scientific lottery. Researches definitely directed to the improvement of scientific methods of known utility, or to the extended application of fruitful methods, might be reasonably advocated; but vague hopes that, by the creation of a State laboratory, something useful *might* turn up,

or that by an observatory for studying the physics of astronomy, something would be learned about the sun or the moon, stars, planets, comets, or meteors, which *might* advance the material welfare of the human race, are scarce worthy of serious consideration. We must limit our attention to the philosophic interest of discoveries to be effected in such laboratories or observatories, unless it can be shown definitely (which no one pretends) in what manner the researches to be conducted may be expected to lead to results of real utility.[1]

But here we cannot but notice how those advocates for the lavish endowment of science, whose opinions have been most strongly brought to bear on the late Commission, have apparently recognised the necessity for suggesting that useful consequences may flow from inquiries which have long been pursued solely because of their scientific interest. Thus we have such vague promises as appear in the follow-

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[1] It will of course be understood that I am here speaking only in the sense in which the vast majority of the taxpayers, *out of whose pockets, be it remembered, the money for these projects would come—* might be expected to view any wide scheme for the endowment of science. As a student of science myself, I should consider that even very large sums devoted (honestly) to the advancement of pure science, without direct reference to material benefits, would be exceedingly well bestowed. But we must remember that it is the opinion of the majority, not the opinion of the scientific few, which would have to be taken if the question of taxation for such purposes could actually be voted upon. If a body of skilful engineers had to decide about devoting their own money to some engineering scheme, they could fairly vote according to their views of its advantages or disadvantages; but if they had been appointed by a large body of shareholders to inquire into the scheme, and though satisfied themselves of its advantages, found themselves quite unable to satisfy those shareholders, it would be thought very unfair if they tried to override the objections which the shareholders were entitled to make, and carried out the scheme despite the wishes of those whose money they expended. They might be abundantly right, and therefore the shareholders altogether wrong, but the injustice would remain unchanged.

ing passage from an address delivered at the last meeting of
the British Association :—'It cannot be doubted that a
great generalisation is looming in the distance—*a mighty
law we cannot yet tell what, that will reach us we cannot yet
say when.* It will involve facts inexplicable, facts that are
scarcely received as such because they appear opposed to
our present knowledge of their causes. It is not possible,
perhaps, to hasten the arrival of this generalisation beyond
a certain point ; but we ought not to forget that we can
hasten it, and that it is our duty to do so. It depends much
on ourselves, our resolution, our earnestness, on the scien-
tific policy we adopt, as well as on the power we may have
to devote ourselves to special investigations, whether such
an advent shall be realised in our day and generation, or
whether it shall be indefinitely postponed. If Government
would understand the ultimate material advantages of every
step forward in science, however inapplicable such may
appear for the moment to the wants or pleasures of ordinary
life, they would find reasons, patent to the meanest
capacities, for bringing the wealth of mind, now lost on the
drudgery of common labours, to bear on the search for
those wondrous laws which govern every movement, not
only of the mighty masses of our system, but of every atom
distributed throughout space.'

These vague promises of mighty material results, of
mighty laws ' we cannot yet tell what ', which are to ' reach
us we cannot yet tell when,' were even more conspicuous in
the advocacy of a scheme for ' an observatory for studying
the physics of astronomy.' ' Permanent national provision
is urgently needed,' said Lieut.-Col. Strange, a Fellow of
the Astronomical Society (who first conceived this scheme),.
' for the cultivation of the physics of astronomy. If the
study of the sun alone were in question, that alone would
justify such a measure ; for there can hardly be a doubt.
that almost every natural phenomenon connected with

climate can be distinctly traced to the sun as the great dominating force, and the inference is unavoidable that the changes, and what we now call the uncertainties of climate, are connected with the constant fluctuations which we now know to be perpetually occurring in the sun itself. The bearing of a vast array of problems connected with navigation, agriculture, and health, need but to be mentioned to show the importance of seeking in the sun, where they doubtless reside, for the causes which govern these changes. It is indeed my conviction that of all the fields now open for scientific cultivation, there is not one which, quite apart from its transcendent philosophical interest, promises results of such high utilitarian value as the exhaustive systematic study of the sun.'

As this is a typical instance of the promises held out by advocates of the new schemes, it may be well to consider it at some length. It need hardly be said that science warrants none of the expectations mentioned by Lieut.-Col. Strange. It is believed by several astronomers that the eleven-year cycle in which sun-spots wax and wane in number and frequency on the solar globe affects the condition of our globe with respect to temperature; but while some stoutly assert that the heat is greatest where there are no spots, others affirm with equal confidence that at those times the heat is least. Some meteorologists, again, consider that the rainfall is modified during the spot-cycle in this subtle manner, that the proportion of rain with south-westerly winds to rain with north-easterly winds is greater or less according as the spots are more or less numerous. But one law is found for England (Oxford) in this matter, and a contrary law in Russia (St. Petersburg), so that it is surmised that somewhere between Oxford and St. Petersburg there is no effect. Later observations suggest that this 'somewhere' is shifting, and therefore necessarily the boundary line to which it belongs is shifting also. In other

words, the results are such as would suggest to any but a
very earnest theorist, the conviction that there is no asso-
ciation at all between the rainfall and the spot-cycle. In
like manner cyclonic storms have been imagined to have
an eleven-yearly cycle, though it is not yet settled whether
storms are more or less numerous when the sun is most
spotted. The electric condition of the earth has been
more definitely associated with the spot-cycle—in fact, this
is the only relation which seems pretty generally admitted.
Yet even the existence of this relation is flatly denied by so
high an authority as the Astronomer Royal. And even if
all these relations were certainly established, instead of
being for the most part doubtful, they would in no sense
tend to establish Colonel Strange's amazing proposition.
For not one of these relations is of the slightest ' utilitarian
value'—whatever its philosophic interest might be, suppos-
ing it demonstrated instead of imagined. And even if these
relations, instead of being quite without material value,
were of some use, yet as they depend on a solar peculiarity
quite striking and obvious, they could prove nothing
respecting the relatively most subtle solar changes which
yet remain to be detected. As the facts actually are, it is
proved to demonstration that no ' utilitarian value ' what-
soever can exist in discoveries respecting the laws of solar
change. For it is shown that a solar cycle far more remark-
able even than others since discovered, and *à fortiori* sur-
passing in importance those which have hitherto escaped
detection, has either no influence whatever on the pheno-
mena of weather, or none of sufficient importance to be
worth knowing *on account of its ' utilitarian value.'*

It is not, however, merely the unsoundness of the
scientific views thus advanced in support of the scheme
for a physical observatory of astronomy that I would in-
dicate. A far more important point is suggested by this
particular instance of unwise advocacy. When we find

that the Royal Commission to some degree adopts the
suggestion that material benefits may be derived from the
study of solar physics, we might naturally assume that
this idea had at least received the sanction of the leading
astronomers of England, and that the opinion of the Com-
mission was based upon their evidence. Should it appear
that this was not the case in this particular instance, we
should be led to inquire (somewhat wonderingly) on what
principle the evidence for the guidance of the Commission
in this and other matters had been selected. Now it so
happens that this very question of an observatory for
studying the physics of astronomy was brought before the
Council of the Astronomical Society with the express pur-
pose of obtaining their influence in favour of the scheme.
No arguments that could be urged in its support were
omitted. The question was adjourned from one meeting
of the Council to another, including two meetings specially
convened for the discussion of that matter alone. Yet,
when the question was put to the vote, only four votes
were given in favour of the scheme ; the majority against
it including (1) the leading official astronomer in England,
(2) the greatest master of the physics of astronomy, (3)
the ablest mathematician in Europe, (4) the private as-
tronomer who, next after the late Lord Rosse, has pene-
trated farthest into the celestial depths, and many other
well-known astronomers. The minority of four included
the author of the above-quoted passage, and two other
ardent advocates of the scheme for a science ministry and
the lavish endowment of science. At the next meeting of
the Council these three resigned their seats on the Council,
stating as their reason the Council's refusal to support
their scheme. When we consider these facts in connection
with the circumstance that the Royal Commission received
scarcely any evidence on the subject save from members
of the defeated minority, we perceive that strong influ-

ence must have been exerted in this special instance to
secure a favourable verdict from the Commission. And
thus some doubts are suggested as to the quality of the
evidence on which the other opinions of the Commission
were based,—not, I need hardly say, that we need question
for a moment the *bona fides* of the evidence, but simply
that we seem justified in doubting whether evidence un-
favourable for particular projects was admitted as readily
as evidence in their favour, if admitted at all.

It remains that we should consider how far it is desir-
able that State support should be afforded to students of
science. Hitherto the pecuniary assistance granted to
students of science by the State or by learned societies
has been limited to expenditure actually incurred for appa-
ratus, and so forth. Only a few fortunate individuals—or
rather, perhaps, a few who have been exceptionally skilful
in making requests—have received even so much assist-
ance. But it is now urged that the science labourer should
receive something more than the mere price of his tools—
that, in fact, since he gives his time and labour to the work
of research, he should be remunerated just as any other
worker would be.

I may note, at the outset, that this part of the sub-
ject is closely associated with the last. In fact, we are
now to consider the personal cost of scientific researches,
whereas before we were considering their material cost.
The same general reasoning is applicable to one case as to
the other. If the nation has the right and should have
the power to select what kind of work it will pay for, it
has an equal right and should have equal power to decide
whether any given class of workers shall receive State aid.

We must not mix up this question with the general
question, whether the student of science should receive re-
muneration, or—to speak plainly—should work for money.
A great deal of nonsense is sometimes heard on this point.

People who do not think it strange or wrong that the minister of religion should be paid for his work, who can even contemplate with considerable satisfaction the princely incomes of some of our prelates, will often speak of the student of science who earns money by means of his scientific knowledge as though he were degrading science. Of such a man they will say, as if some special disgrace attached to the words, he is trading on his scientific knowledge; yet they would be startled (so great is the difference that custom makes) if anyone were to say of a church dignitary that he was trading on his theological learning or on his spiritual experience. And sundry stories are related to show how the true lover of truth has behaved. We are reminded how Faraday despised the thousands which he might have gained by turning his physical experience to trade purposes; how Agassiz, when offered high reward for imparting knowledge, said he had no time to earn money, and so forth. It is forgotten that, in these and other instances, eminent men of science have merely sacrificed superfluities, or, rather, they have justly valued their opportunities for scientific study at a higher rate than other luxuries which money could bring them. Faraday had ample means, and made ample provision for his family; Agassiz, if he could not spare time to earn money, permitted his wife to earn it for him by teaching; and it has not yet happened, so far as I know, that any student of science has been so unduly zealous in the search after truth as to insist, for its sake, on remaining poor, and on bringing want upon his family. There is no reason why he should do so, while there are abundant reasons why he should not. As Huxley has said, the student of science is 'not only a citizen, but he is a citizen in the first place, and a student of science in the second.' If he is a true lover of science, the study of science affords him profound pleasure, and this pleasure is well worth the sacrifice of a good deal

of money; but as a citizen he is bound to ask how much
money he can afford (either to pay or to sacrifice, it matters
little which) for that pleasure. Duty must come first, and
if duty towards himself and his family requires that he
should earn money, he must, *pro tanto*, sacrifice science.
Should it so chance that science drudgery — teaching
science, writing or lecturing about it, making practical use
of it, or the like—affords an available means of earning
money, he may be thankful it is no worse, and that duty
does not compel him to abandon science altogether. But
assuredly he is open to no blame for doing his duty first,
and considering pleasure (the great pleasure of his life,
that is,—the pursuit of truth) afterwards.

While it is manifest, however, that the student of
science, like his fellow-citizens, must earn money, unless
he already possess it, and must (in these times) try to
earn a good deal of money if he has many to maintain
besides himself, and while it is equally manifest that his
scientific labours would progress much faster if he were
relieved from this necessity, it by no means follows that on
that account the nation ought to be taxed for his support.
The nation, as I have already pointed out, has a right to
select what it wants, and the conviction of a few or even
of many men of science that excellent results would follow
if science were lavishly subsidized, is not a sufficient reason
for taking money from taxpayers, the vast majority of
whom would object to pay the money for that purpose if
the questions were plainly submitted to them.

But there is not, in point of fact, that unanimity of
opinion among scientific men on this point which some
have asserted. It is indeed very gravely questioned by
many of the most eminent men of science in the country,
whether if large sums were devoted to the endowment of
science, the right men would get the money. The men
who would be readiest to thrust themselves forward when

money was offered for scientific research would not pro-
bably be the men to whom scientific research is a delight.
It is not everyone who studies science, nor even is it every-
one who has done good work in science, who is possessed
by the true scientific spirit.   It is not wholly impossible
that some who, on the strength of past services or per-
chance one solitary success, might thrust themselves into
the most forward positions, would not be found ready to
continue the arduous prosecution of scientific labours when
once they found themselves placed in well-paid offices.[1]

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[1] I have before me a letter written by Professor Holden, of the
Washington Observatory, which shows how Americans view a question
which their practical good sense renders them especially well able to
deal with.   I shall venture to quote some passages from this letter,
premising that though I feel sure Professor Holden would take no ex-
ception to my so doing, the letter was not intended for publication. (It
bears date June 2, 1875): 'It has, I confess, been a wonder to me,' he
writes, 'how the endowment of research could be seriously advocated by
anyone who had considered what the practical outcome would be.   It is
faultless in theory, but practically science would suffer if the restric-
tions were removed which have grown up about the greedy. . . . I
take pride in Young's researches on the sun, done in the midst of class-
room work, and am prepared to believe that the whole work of the man
helps science along more than it would if New Hampshire had made
him Government astronomer and spectroscopist in ordinary.   Fancy
our Congress endowing research!   We are very well off.   We live
under the despotism of poverty, tempered by the Smithsonian Institute.
. . . I doubt, too, if England could manage the distribution of any
large sum among the proper men.   Some of the most eminent ought
not to have a share, since they are lazy, and some of the hard-working
lack brains.   How would one go to work to pay Burnham or Dem-
bowski' (distinguished observers and measurers of double stars) ' for
their work ? '   When we remember the singularly rapid progress which
science has made and is making in America, and the readiness with
which the American Government has responded to proper appeals for
assistance to science (as in the recent eclipse expeditions, the transit
expeditions, and still more in scientific researches promising results of
material value), we cannot attribute to any deficiency of zeal or libe-
rality the objections which Americans entertain against schemes for the
endowment of research.

Of course, a good deal would depend on the manner in which candidates were selected, and on the kind and degree of supervision exercised over the holders of various scientific offices. Competitive examinations, whether justly or unjustly objected to in the case of naval, military, and civil service appointments, would assuredly be required for the new scientific service. Mere popular repute would be a perfectly useless criterion. A man may be popularly reputed an authority in chemistry who could not be trusted to effect the most ordinary analysis, or in geology, whose whole knowledge of the science has been derived at second-hand from its real masters. The public may regard as an eminent astronomer a man who has not really mastered the merest elements of mathematics, or as a most skilful physicist one who has but made ingenious use of the discoveries which others have laboriously worked out. Nor can any reliance be placed, as so many imagine, on the fact that a student of science is a member of many learned bodies in our own country or abroad. Such honours are open to almost anyone who is prepared to take the proper course to obtain them; that course being such as some science-workers consider not wholly consistent either with self-respect or with the dignity of science. And unfortunately there is reason for believing that if the appointments to scientific offices were left to ministerial responsibility, personal recommendations not entirely based on the scientific profundity of the candidate for office, would come into operation. An examination conducted by professors from our universities and scientific institutions (in other words, by men already thoroughly tested by long service), would obviate these objections, though probably considerably narrowing the field of selection, for many who would very readily accept some of the offices which the new scientific ministry might create would be somewhat unwilling to undergo the test of examination in those

departments of mathematics and physics with which every thorough student of science should be familiar. It may even be questioned whether the men really competent to undergo such an ordeal would not be likely to have already achieved such success as would render the best paid offices under the new ministry scarce worth their acceptance.

Another point to be mentioned rather affects the interests of science than those we have hitherto chiefly considered—the interests, namely, of the tax-paying community. It has hitherto been found that in the civil, military, and naval services, the rule has come into operation which Herbert Spencer has called the 'ineradicable vice of all services,' the rule of putting young officials under old, and thus 'placing the advanced ideas and wider knowledge of a new generation under control of the ignorance and bigotry of a generation to which change has become repugnant.' As Herbert Spencer well remarks, this 'which is a seemingly ineradicable vice of public organisations, is a vice to which private organisations are far less liable; since in the life-and-death struggle of competition, merit, even if young, takes the place of demerit, even if old.' I need not insist on the mischievous effects which would follow if those men of science who, under the present system, would enter on a line of research of their own, were, merely for the sake of some fixed salary, to accept office under an old chief whose antiquated views they would be expected to carry out. Already, even with the present comparatively limited scientific service, we see the effect of such arrangements. We see also the still more mischievous effect of another peculiarity of public service, the fact that men reserve their powers (even to the extent of doing absolutely nothing) in anticipation of their appointment to offices not as yet vacant, while the holder of the office, perhaps already too old for efficient service, carefully

reserves such powers as he still has, in order that he may hold the office as long as possible.

The proper course, as I have elsewhere indicated, would be 'to proceed tentatively. It is almost certain that any general scheme formed at the present time would hereafter have to be largely modified, if not wholly abandoned. The time has not yet arrived when the nation would look with satisfaction on any wide scheme of scientific endowment, even if Parliament could be persuaded to make adequate grants for such a scheme, or to authorise the employment for that purpose of funds available at the two universities. . . . . . The nation is probably not unwilling to see experiments made on the effect of endowment for special scientific purposes. If such experiments were made, we should gradually perceive whether wider schemes were likely to be advantageous to science, or whether dangers may not lurk in all such schemes. It might be found that endowment would greatly tend to increase the number of those entering on scientific pursuits, while widening also the range of scientific culture. Or, on the other hand, it might be found that the national endowment of science would tend only to advance scientific Micawberism, and that the real workers in science would be discouraged by seeing all the best rewards given for pretentious novelties, clever adaptations, perhaps, of their own discoveries. . . . . . Practical experience has, indeed, already taught that dangers, and serious ones, surround such schemes. . . . . . Before long, however, the real position of affairs will be known. If the present desire for the endowment of research is prompted by genuine zeal for science, we shall find that the warmest advocates of the scheme are not those who would themselves profit by it. But if, on the other hand, it should appear that the persons who now speak most earnestly about the endowment of,

science are in reality eager chiefly for their own preferment or wish to secure posts of emolument for personal friends and adherents, then every lover of real science must desire the failure of such schemes, seeing that the cause of science would not fail to suffer, nor science herself to be degraded should they prove successful.'